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THE
BRITISH JOURNAL
OF
PSYCHOLOGY

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THE BRITISH
JOURNAL OF PSYCHOLOGY

PSYCHOLOGICAL ANALYSIS AND THEORY OF HEARING.

BY HENRY J. WATT.

I. The background of senses on which hearing stands.
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VI. Physiological theory of hearing.
Formulation of a new theory 'on demand'; its relation to the theories of Helmholtz, Ewald, and ter Kuile, and to the main groups of psychological facts.

DURING the last few years rapid additions have been made to our knowledge of auditory sensations, their relations, and their causes. And the natural product of the new facts has come forth in various extensions and modifications of previous theories. One of the most striking features of these is the eagerness with which inspiration is borrowed from the visual system of qualities. But we must remember that vision is only one of the senses and in taking its complexity as a model we may fail to do justice to the claims of the other senses. I have attempted to satisfy these claims by a general consideration of all the senses and in doing so I was compelled to advocate a change in our treatment of sound[1]. It is commonly held that the attribute of quality in the sense of sound is represented by what we commonly call pitch. I proposed to see in this, however, not quality, but the auditory analogue of what we call 'local sign' or localisation in the cutaneous and visual senses. The attributes of sound were then to be: quality, of which, as in the sense of touch, there is only one form; intensity; pitch, generically named 'order'; voluminosity, the parallel of extensity in other senses; and the temporal attributes. On careful consideration of all the new facts and theories I venture to say that the extensions of auditory theory which build upon the common treatment of pitch as quality and which confine their search for inspiration by analogy to the sense of vision with its manifold qualities are entirely misleading and that the best guide to a theory of sound, as of every sensory experience, simple or complex, is a general consideration of the typical characteristics or attributes of all the senses in parallel, as far as they go. In this paper I propose to show that the analysis of auditory sensations I suggested is not only preferable to any other present theory, but is the only acceptable line of analysis.

I. *The background of senses on which hearing stands.*

'Midst the details of our knowledge concerning the complex senses it must not be forgotten whence we obtain our pattern for their analysis. We certainly do not find it in vision, which itself needs more help than it can give. Our standard lies rather in the simplest of sensations. In them we read both the problem and the solution of all the others. These may be divided into two groups, making with the first group three in all. A brief statement of these three groups shows how the problems of hearing, which belongs to the third group, must be approached.

The first group consists of the cutaneous and visceral senses. There is no difficulty in identifying the attributes of all these. In spite of the existence of different systems of receptors for each sense, only one quality occurs in each sense. It may differ in intensity. But one quality in varying intensities would only constitute one sensation, not a sense. That is provided by the occurrence of differences of locality or local sign, which is an ordinal attribute. Along with it goes a continuitive attribute, here known as extensity. By that is not meant a variable feature of area or surface, but a constant constitutive of area of any kind. Only by a variation of order along with constancy of extensity can a variable extent be thought to occur. In the cutaneous senses the minimal sensations we actually receive are very small extents and present practically no room for further differentiation of localities, at least as far as our phenomenal distinctions go. The temporal attributes may be neglected in this paper[1].

---

[1] See This *Journal*, 1911, iv. 143 ff.

Sensations of taste may be added to this group. Their attributes are identical with those of the cutaneous sensations in all respects but one, viz. that in taste we find four different qualities. But this difference presents no real difficulty. We have almost as much reason to see in the four qualities of taste four different senses as in the four different qualities of cutaneous sensation. What these reasons are need not be recounted here. At the same time we may readily admit that the four gustatory qualities are more akin to one another than are the other four. Thus arises the suggestion and the problem that the sensory qualities occur only in forms which differ from one another by a distinct step. The solution of this problem must be the preliminary to any problem as to the nature of the difference between qualities.

The second group of senses differs from the first clear and easy group in being obscure and somewhat difficult in the matter of attributes and in presenting a certain amount of complexity. The senses included in it are the muscular, articular and organic. The obvious form of variation of the first is intensity, while locality, extensity, and quality are all obscure. Quality is easily absolved by the recognition that, like those of the senses already mentioned, it does not really vary. Locality or order varies from muscle to muscle, but not for any one muscle; and extensity is contained in the variation of the mass or bulk of the sensations which come from muscles of different size. In the articular sense we find a similar group of features, differently distributed however. For, while quality is likewise single, it is intensity which is obscure. The reason for this is probably merely want of variation[1]. The only obvious variant in articular sensation is 'position,' which we may take to be its ordinal attribute. Extensity behaves here as in muscular sensation. It is implicit in the varying bulk, mass, or volume of the sensations from the joints, small and large. In organic sensations we find that obscurity and want of variation is fairly general, but with a little willingness all the attributes can easily be identified.

---

[1] For a detailed account of the distinction and justification of these attributes, see my previous papers in This *Journal*: (1) "The elements of experience and their integration," 1911, IV. 135 ff.; (2) "Are the intensity differences of sensation quantitative?" 1913, VI. 176 ff.; (3) "The main principles of sensory integration," *ibid.* 240 ff.

Two psychological problems are raised by the sensations of this group. The first is that of the presence and cause of the obscurity of an attribute. In these cases it may be referred readily enough to the absence of variation in the physiological correlate. Thus the organs of the articular sense are stimulated at all times by forces of practically constant intensity, while the organs of the muscular sense, if there are enough of them in each muscle to form a small system there, are all stimulated at once, not one at one moment and then another.

The second problem is that of compound sensations. In all the senses of this group we have good reason to suppose that many receptors are stimulated at once, as there are many in each muscle, round each joint, and in each proprioceptive mechanism of the body. This fact confirms the psychological conclusion that must be drawn from the mass, bulk, or volume that is inseparable from the corresponding sensations. Apparently we never get in isolation a single 'spot' of articular, muscular, or organic sensation, but only a mass or area of it. Area is familiar to us in cutaneous sensation and there is every reason to suppose that area consists there of a large number of neighbouring minimal (as we get them) sensations, which fuse to a continuous whole in virtue of the extensity that each possesses. There is in area no accentuation or discrimination of orders, unless by means of concomitant differences in the variable attributes of intensity, as also sometimes of quality (vision)[2].

This theory of compound sensations may be extended to apply to the minimal sensation from the 'spot' of cutaneous sense, which may well be supposed to be only minimal for us, because the units of the receptor organs or their neural attachments make a smaller particulation of sensation impossible, and not because the 'spot' sensation does not really consist of still smaller psychological particles fusing into a tiny area in which no orders can be discriminated[3].

---

[1] Cf. This *Journal*, 1911, IV. 159.

[2] Cf. my paper on "The psychology of visual motion," This *Journal*, 1912, V. 32 ff.

[3] Cf. the psychological theory of intensity offered by F. Brentano, *Untersuchungen zur Sinnespsychologie*, Leipzig, 1907, 53 ff.

The third group of senses raises all the difficulties and problems already mentioned and some new ones. The senses included are vision, hearing and smell. We find in the matter of attributes special cases of obscurity, difficulty, and complexity. The obscurity of intensity in vision raises a general problem regarding the primacy of intensity as an attribute and regarding its place amongst experiences, if it be secondary or derived[1]. The manifold and continuous variation of colour quality, as surveyed in the colour figure, raises anew the problem of compound sensations. Both of these appear again in the study of auditory sensations, which adds as a third the problem of a non-spatial, systemic order and continuity. In this paper I shall confine my attention to the problems of hearing.

II. *Views concerning the quality of sounds.*

The feature of auditory sensation which till very lately has been generally classed as quality is pitch. As it varies continuously from lowest to highest tones, the number of qualities equals the number of distinguishable pitches. The smallest number of primary qualities to which this series of continuous variations can be reduced is two, of which each occupies one end of the phenomenal series. Such a reduction which treats the series of tones as the analogue of the series of visual brightnesses, was suggested by Mach[2]. But it is rejected by Stumpf and is not generally admitted. Each distinguishable tone is rather considered to be a simple elementary sensation, requiring, as in Helmholtz's view, a specific sense-organ. The number of these is therefore very great. Ewald has pointed out that the assumption of this correlation between a vast series of qualities and a vast series of sense-organs puts an enormous strain upon our conception of the biological evolution of hearing[3].

Brentano[4] thought to remedy the deficiency of Mach's theory, while retaining its postulate of tonal primaries, by extending the analogy with vision. He therefore recognised in hearing a series of 'saturated elements'—the tones that lie within an octave. The repetition of octaves was to be explained by reference to variation of the admixture of the brightness components of sound. But this theory, like Mach's, could not hope to be accepted on its own merits. For, although it explains various matters well enough, as any such theory may, it does not add enough to our insight and to our knowledge to make itself compelling, and it is not founded upon a mass of observations, as is that lately propounded by Révész and von Liebermann[1]. Their theory is practically identical with that of Brentano, except that what he calls difference of brightness, they call difference of *Höhe*[2]. But the observations made by von Liebermann on his own auditory sensations, as modified by a chronic paracusis, allow these authors to go far beyond the range of Brentano's views and make their theory much more convincing. For they have established the independent variation and recognition of these two aspects of tone. Their observations seem to me to be thoroughly consistent with all the known phenomena of hearing and therefore *prima facie* correct. Only their classification and theory of these aspects of tone appear to me to be impeachable and misleading. Their distinction explains much, of course; but as its basis in fact seems to be correct, proper classification and proper deductions therefrom should enable their theory to explain still more.

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[1] Cf. F. Brentano, *op. cit.*, and the physiological theory of intensity proposed by C. S. Myers, This *Journal*, 1913, VI. 137 ff.

[2] *Beiträge zur Analyse der Empfindungen*, Jena, 1886, 122. In this edition Mach uses the analogy of the series leading from red to yellow. This is in principle the same as the series leading from black to white.

[3] J. R. Ewald, *Arch. f. d. ges. Physiol.* 1899, LXXVI. 155.

[4] *Op. cit.* 101 ff.

It is to be noted that Max Meyer[3], whom Révész also quotes[4], distinguishes the same two aspects of tones, but he calls them by reverse names. Révész's quality is his pitch and Révész's height is his quality.

In the hands of Köhler[5] and more especially of Jaensch[6], Stumpf's generally accepted views have taken a different line of development. The chief influence here has been the observation of the resemblance between pure tones and vowels and also in the case of Jaensch consideration of the nature of the relations between the stimuli of tones, vowels and noises. The pure vowel sounds are for Köhler the sole qualities of hearing. For Jaensch they are only the qualities of the sense of noise. He recurs to a modified form of the analogy between vision and hearing in identifying vowels and brightnesses as less differentiated sub-senses. The stimulus for the former is a rate of vibration which varies irregularly but not too extensively about a certain average, while the stimulus for the latter, although not necessarily irregular, is at least in the normal, light-adapted eye usually provided by certain pairs of lights or any mixture of these pairs. The more differentiated and presumably later developments—tone and positive colour—are evoked by steady rates of vibration and are likewise to be identified. The relation between tones and vowels, like those between colours and brightnesses, varies, sometimes being closer than at other times. Thus, presumably, we should have to admit a whole spectrum of qualities in the tones, and a series of qualities in the vowels—as it were octaves of brightness. Why there are different qualities in this series is not explained. And as Jaensch distinguishes vowels from noises, I can see no reason why we should not admit three sub-senses in hearing instead of merely two[1].

---

[1] Cf. the works quoted below under their names.

[2] This is the ordinary German word for our 'pitch'; for the sake of clearness, however, I shall translate it by its wider meaning—'height.'

[3] "On the attributes of sensations," *Psychol. Rev.* ix. 83, esp. 95 ff.

[4] *Zur Grundlegung der Tonpsychologie*, Leipzig, 1913, 42.

[5] *Ztsch. f. Psychol.* 1910, LIV. 241 ff. and 1911, LVIII. 59 ff.

[6] *Ztsch. f. Sinnesphysiol.* 1913, XLVII. 219 ff.

The analysis into sub-senses is hardly attempted at all by Révész[2]. The only indication of it we get is the suggestion that the height of tones would need two psychophysical processes, while quality, as the evidence of binaural mixture of small pitch differences seems to show[3], would also probably require two. If we add to these the differences of vocality which Révész recognises[4], still more processes will be required and we shall quickly exhaust the resources suggested by even the most complex of visual theories.

The enormous influence of the analogy with vision upon these theories is obvious. I shall now proceed to summarize the facts and observations included in them and to interpret them according to my own theory of the attributes of sound stated above. It will then be evident which of all theories is the more systematic, that is free of difficulties and full of promise.

---

[1] Cf. *op. cit.* 240, 255, "Weichen die Schwingungszahlen der einzelnen Elemente eines Kurvenzuges allzu stark von einander ab, so wird aus dem Vokal ein Geräusch....Das undifferenzierte Geräusch ist somit die eine Klippe, welche bei der Herstellung eines Vokales vermieden werden muss; die andere Klippe ist der Ton." If there are only two cliffs, there must be a valley between them, namely vowels. Or are there really three cliffs?

[2] *Zur Grundlegung der Tonpsychologie*, 41 ff.

[3] *Nachr. d. Gesell. Wiss. Göttingen*, Math.-phys. Kl. 1912, 676 ff.

[4] *Zur Grundlegung der Tonpsychologie*, 89, "Es gehört eben jeder Schwingungszahl eine Qualität, eine Höhe, und eine Vokale zu."

III. *The relations of tones, vowels, and noises.*

There are pure tones and there are compound tones. The latter consist of many simultaneous tones which harmonize more or less with one another and with their fundamental component. If a tone does not last longer than the time of two vibrations, it is heard as a noise. Many tones of neighbouring pitch or generally inharmonic in relationship, sounded together, form a noise. The pitch of single noises is not very evident. But many noises contain distinguishable tones, and if noises are sounded one after another, their pitch becomes easily noticeable. Thus we obtain the propositions: (what is presumably) tone is sometimes heard as noise; some noises consist of (what are presumably) tones; some noises contain tones. The net result of these is the proposition: many (phenomenal) noises are, or consist of, (real) tones. In reliance upon this, it has been usually inferred that all noises consist of tones or are tones of very indefinite pitch or are not yet tones, so that there is no special sense of noise[1]. This conclusion seemed to be confirmed by the rather vague and conflicting statements made about the tonal nature of vowel sounds. The synthesis of sounds by experimental means did not admit of a direct examination of the proposition: are there any unanalyzable noises of indefinite pitch?

This experimental question has been answered by Jaensch by the use of a selenium cell placed in the circuit of a telephone and illuminated by an arc lamp whose light was varied by the revolution of an obstructing disc. The edge of this disc was cut out so that the variations in the length of its radius corresponded with the variations in height of any desired vibratory curve, pendular, periodic, or irregular. The results of these experiments are most acceptable. A constant rate of vibration produces a tone. The same average rate of vibration produces a vowel-like tone, if the mean variation from the average is still small. As the mean variation increases, the sound passes gradually into a vowel, then it takes on a noisy character, and when the mean variation is great enough, it may finally appear as pure noise. The average rates of vibration of the vowels *m, u, o, a, e, i,* and *s* are approximately octaves of one another[2]. These vowels do resemble certain tones, as Köhler maintains[3]. But they are not to be identified with them, as he proposes. The resemblance is close only between the lowest and highest vowels and the corresponding tones; in the middle of the scale, from below the vowel *o* to above the vowel *e*, tone and vowel are easily distinguishable. Jaensch, therefore, ascribes vowels to a separate and older sense of noise, of which he supposes them to be primary qualities. Average rates of vibration that are greater than that of any vowel and less than that of the next higher one, form a series of mixed vowel sounds, which show a decreasing resemblance to the lower vowel and an increasing resemblance to the next higher vowel, as the average pitch rises. The changes from pure vowel to pure vowel thus obtained are parallel with the changes encountered as we pass from red to yellow, etc.[1]

---

[1] Cf. C. S. Myers, *Textbook of Experimental Psychology*, 2nd ed., Cambridge, 1911 25 f.

[2] Jaensch, *op. cit.* 234 ff.

[3] *Op. cit.* esp. LVIII. 91 ff.

Two statements of great psychological importance are involved in these views: (1) that hearing contains two psychologically independent sub-senses—tone and noise; (2) that *u*, *o*, *a*, and the rest are pure vowels, forming a series of qualities in the sense of G. E. Müller[2].

The following objections have to be urged against the distinction of two sub-senses. It is supported by nothing more than analogy, and, at its best, that analogy is the analogy of stimuli, not of experiences. The stimuli of vowels and noises are irregular, those of tones are regular; the stimuli of colours are regular, those of neutral greys irregular. But it is to be noted that, while the former vary round an average, the latter go in pairs—those of the complementary colours. It is known that the sub-senses of vision exist independently; but there is no evidence that the sense of noise can exist without the sense of tone. And if there were such evidence, it would not be clear of ambiguity; for noises are not only, *ex hypothesi*, excited by tones, as brightnesses are excited by colours, but noises, when given, *ex hypothesi*, alone, resemble tones. It is true, as Jaensch propounds[3], that each positive colour has an affinity to a neutral brightness, but it is not true, as his diagram suggests, that each neutral brightness has a resemblance to a colour, *quâ* colour; whereas each tone resembles (or according to Köhler is) a vowel, and on Jaensch's analogy must resemble it, because it excites it, and also each vowel resembles (or according to Köhler is) a tone or has the pitch of a tone, as Jaensch[4] has shown experimentally, and as all those who have attempted to find the component tones of vowels have observed. Moreover increase of intensity of light modifies a colour in the direction of greater neutral brightness, but increase of the intensity of sound does not bring a pure tone nearer to either vowel or noise. The sound-figure given by Jaensch[1] should be made tri-dimensional to suit the double parallel between the similarity of low and high tones to vowels and the varying similarity of different hues of different brightness to neutral greys, just as the tri-dimensional colour figure does. But this cannot be done for tone. The analogy of vision and hearing, tempting though it be, is both incomplete and misleading.

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[1] Köhler, *op. cit.* LVIII. 99; Jaensch, *op. cit.* 258 f.

[2] *I.e.* "Eine Reihe von Empfindungen, in welcher sich die Qualität geradläufig [d. h. in konstanter Richtung vor sich gehend] und stetig ändert," *Ztsch. f. Psychol.* 1896, x. 33 ff.

[3] *Op. cit.* 264 ff.

[4] *Op. cit.* 288.

In any case, what sort of statements do the facts precisely warrant? The stimuli used show that, as we pass from tone through vowel to noise, the average rate of vibration remains constant, but its mean variation increases; in other words, the pitch wobbles; not markedly and noticeably, but none the less truly. We become less able to indicate the pitch by singing, or by naming it. But we can approximately find it (with a little circumspection and comparison) the more easily, the more it is isolated from accompanying (not from successive) tones, vowels or noises[2]. I see no reason to depart from the substance of the formulations regarding the relations of tones and noises referred to above. A noise may then be said to be a simple sound whose pitch is not yet audible or a complex sound of many pitches which make each other indistinguishable to the unaided attention. Or, to put it more briefly: tones, vowels, and noises vary from one another in respect of (decreasing) definiteness of their predominant pitch. Later on we shall see more how this statement can be founded and confirmed by both physiological and psychological theory.

The following objections have to be urged against the view that the series of vowels *m*, *u*, *o*, *a*, *e*, *i*, *s* and *ch* form a series of qualities in the sense of G. E. Müller.

It has long been an accepted view that the tones within the octave are not a mixture of the two end-tones of the octave. If the octave relationship and such others are ignored or suppressed, as for example by running in a single chromatic series through several octaves, all the tones passed seem to form part of a single series. If we suppressed the connexion between the octaves, as it holds in the slightly indefinite pitches of vowels, by somewhat similar means, should we not seem to pass through a single series of vowel sounds, in which there are no turning points? Amongst pure noises, the existence of which, as we have seen, Jaensch, like everyone else, must admit, no octave connexion of any kind is to be found. How then does it come to be present in the parallel series formed in the course of his experiments—pure tones, tone-vowels, and vowels, if not simply in virtue of the character which the stimuli suggest—the more or less precise average value of the vibratory frequency, the more or less definite pitch of the tones and vowels? The purity of the members of the vowel series would therefore by presumption rest upon the general octave relationship of sounds and not upon changes of quality in Müller's sense.

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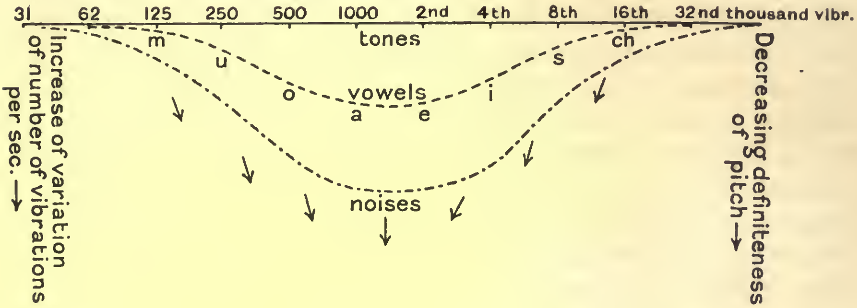
[1] *Op. cit.* 265.

[2] Cf. Jaensch, *op. cit.* 288. A vowel produced apart from a fundamental tone has a pitch. "Die Tonhöhe erscheint hierbei im allgemeinen um so weniger deutlich, je grösser die mittlere Variation der Schwingungszahlen ist."

If the series of vowels consists of greatest differences whose stimuli differ by approximately an octave, we must not forget that the series of tones also contains an octave relationship of the greatest resemblance. It is not easy to reconcile this opposition, unless by the conversion of the 'greatest difference' given in the vowel series into a 'greatest resemblance amidst continuous difference,' just as is generally done for the pure tonal series. In Révész's view there is only a short series of different 'qualities' of sound, that covering the octave. The qualities within the octave differ the more, the greater the interval. Thus the greatest difference—the octave—is converted into the least difference, identity or repetition of quality. Only thus can we harmonize the older sense of noise with the younger sense of tone, as Jaensch calls them. Or surely Jaensch does not think that, if the series of vowels is the parallel to the series of neutral brightnesses, the series of pitches can be the parallel to the series of positive colours in any psychophysical sense? For if it were, we should have to see in the series of pitches a uni-dimensional series of qualities; or else we should have to split the bi-dimensional or tri-dimensional manifold into several uni-dimensional series, as is done in vision. In either case it would be difficult to find pure primary qualities. Even for Révész the method he suggests of deriving all the inter-octave qualities from two qualities is excluded; for his end qualities are greatest similars as well as greatest differences. If he wishes for greatest differences, he must seek them in tones that are just short of an octave apart. But to admit these as greatest differences would compel him to suppose that the greatest differences of all are next neighbours in the tonal series. These minutest of differences do not, of course, act as greatest differences; a slight mistuning of the octave is hardly noticeable. And then again no tones in any part of the octave give themselves out to be purer than any others.

If the vowels of the series are to be taken as greatest differences, the fact that their pitch, in spite of its indefiniteness, is fixed round about an absolute point, becomes important. For the octave relation seems to hold only between the pure vowels, not between impure vowels which differ by approximately an octave. If this be so, it becomes difficult to see why the octave relation should be perfectly relative amongst tones, as it is. To postulate different senses, as Jaensch does, is too easy a way out of the difficulty. For both vowels and noises have a pitch, indefinite though it be.

We must therefore conclude that the series of vowels is a single linear series. If so, it is hardly necessary to bring the relations between tones, vowels, and noises into schematic form. But it might be done thus:


31
62
125
250
500
1000
2nd
4th
8th
16th
32nd thousand vibr.
tones
m
u
o
vowels
a
e
i
s
ch
noises
Increase of variation of number of vibrations per sec. →
Decreasing definiteness of pitch →

The only variant is pitch; it is higher or lower, and it is more or less definite. We have not far to seek for an analogous case of variation in definiteness of order, in the specific form of localisation; it is found in the differences between the epicritic, the protopathic, and the deep systems of cutaneous sensations. Besides a very acceptable physiological basis for increasing indefiniteness can be suggested in hearing.

The only question which remains is to explain why the vowels form a series of absolute octaves. When there is no known reason, any theory formed solely to explain this fact, as Jaensch's and Köhler's are, is as good as any other. Whether we explain octaves by the use of *ad hoc* qualities or not, we still have to explain why in pure tones the octaves are thoroughly relative, whereas for vowels they start from an absolute basis. If this absolute basis be denied, the whole distinction of pure vowels collapses. It is therefore quite as good a suggestion to suppose that the mouth for some reason or other chooses to form such a cavity for some one vowel that it gives a certain average tone, and that the other vowel sounds are chosen, owing to the otherwise and already existing octave relationship, in relation to this primary vowel.

Our conclusion, then, thus far would be that there are in sounds differences of pitch of greater or less definiteness; that the only qualities involved in these differences are those of pitch, if these can be classed as qualities, or, if they are classed otherwise, as I believe they must be, that there are no differences of quality in sound at all; that there is only one auditory quality, namely sound.

IV. *The distinction of two aspects of tone within pitch.*

Various distinctions of this kind have been made. The most familiar recognises the variation of volume or voluminousness which accompanies change of pitch[1]. I think this distinction is also the most correct, and I have therefore adopted it[2]. Low tones are bulky and massive, high tones are thin, sharp and wiry. Sound offers in this respect a parallel to the bulk and volume of the second class of sensations, the obscure group. And we have good reason to extend to them the theory of compound sensations which applies to the articular, muscular, and organic sensations[3].

Max Meyer and Révész also distinguish two aspects within pitch. The former calls them pitch (corresponding to the place in the musical scale) and quality (Révész's 'height'). Révész calls the differences of pitch within the octave 'quality' and an accompanying distinguishable difference he calls 'height.' I accept the distinction intended without the least hesitation, but it seems to me more correct[4] to class the differences intended as differences of 'pitch' and of 'volume.' Pitch, as I mean it, is Révész's quality, such differences as are given within the octave and are named by the letters *c*, *d*, *e*, *f*, etc. But I would not confine the range of these differences to those of a single octave, supposing, as Révész does, that the most similar qualities of different octaves are identical; I would recognise a regular variation of pitch from lowest to highest tones. Volume, as I intend it, is the difference intended by Révész's *Höhe* (height), a difference which changes with pitch throughout the whole range of tones, and which can be separately distinguished from pitch or order, and which can be compared with other degrees of volume. Révész and v. Liebermann have added very much to our knowledge of these properties of tones by showing in such detail that pitch and volume (or, as they say, quality and height), are separately variable, to some extent in normal cases, and so completely in such an unusual case as v. Liebermann's. But I do not think that their theory of the limitation of qualitative differences to those that lie within the compass of an octave, contributes towards the explanation of the octave recurrences of tones. It introduces more difficulties than it removes. It is possible and it is better to place the octave recurrences as a special feature of tone to be explained by reference to the nature of the smallest complexes in which tonal sensations are given to us. To establish this view it will be necessary to make a somewhat detailed study of the facts reported by v. Liebermann and Révész[1].

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[1] Cf. Stumpf, *op. cit.* I. 207 ff., II. 56 ff., 537 ff.

[2] This *Journal*, 1911, IV. 143 ff.

[3] Cf. This *Journal*, 1913, VI. 242 f.

[4] For reasons see This *Journal*, 1911, IV. 143 ff.

(*a*) *The hearing of single tones.* These were heard from a certain point of the scale onwards as a more or less constant pseudotone. The nature of the disturbance varied from time to time, often during one day, and sometimes even during a single observation, then giving rise to a gliding tone. But the main features of the disturbance were constant enough. As I take it, the disturbance consisted in some abnormal process affecting the basilar membrane or the contiguous parts involved in the reception of sounds; in its milder forms this made certain points or short extents within a larger extent of the receptors more sensitive to stimulation than the other parts of this extent; at the same time the effect produced upon the receptor itself was less intense than usual. In its severer forms a part of this region might be totally unreceptive. An analysis of the table given by von Liebermann and Révész[2] shows the following state. In general the region from about e^2 to $d\sharp^4$ or f^4 is affected. On 20th and 27th April, 1907, there is in that region only one sensitive area, $g\sharp^3$ (as such and *quâ* g^2, g^4), with sometimes a semitone higher and lower owing to assimilation or the halving of differences. On 17th October there are two almost equally sensitive points, $f\sharp^3$ and c^4 (also *quâ* f^2 and $f\sharp^3$, or c^3 and c^4); of these f is affected by tones above f and below c in scale, while other tones affect c; the region from $c\sharp^4$ to e^4 was dead. On October 29th the more sensitive point was g^3 (also *quâ* g^2, g^4), with some assimilation to a^3; c^4 was also sensitive. The same holds true of November 5th, only the dead region is now slightly alive. In these last thrice there is assimilation to tones lower than the most sensitive one only if the assimilating tone is on the lower edge of the affected area. The paracoustical region is not, as von Liebermann and Révész suggest[1], broken by normal points, but only by points where the objective tone and the pseudotone coincide. The hypothesis of greater sensitivity fully explains the normality.

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[1] The papers by these authors, Paul von Liebermann and Géza Révész, are: (1) "Ueber Orthosymphonie," *Ztsch. f. Psychol.* 1908, xlviii. 259 ff.; (2) "Experimentelle Beiträge zur Orthosymphonie und zum Falschhören," *ibid.* 1912, lxiii. 286 ff.; (3) "Ueber eine besondere Form des Falschhörens in tiefen Lagen," *ibid.* 325 ff.; (4) "Ueber binaurale Tonmischung," *Nachr. d. Gesell. Wiss. Göttingen*, Math.-phys. Kl. 1912, 676 ff. These will be cited in the following pages as L. and R. 1, 2, 3, 4. Under the name of Géza Révész alone there have appeared: (1) "Nachweis, dass in der sog. Tonhöhe zwei voneinander unabhängige Eigenschaften zu unterscheiden sind," *Nachr. d. Gesell. Wiss. Göttingen*, Math.-phys. Kl. 1912, 247 ff.; (2) *Zur Grundlegung der Tonpsychologie*, Leipzig, 1913.

[2] L. and R. 1, 263.

At a later date (1911) the state of the ear was of the same character, the most sensitive point was about $g\sharp$, but the area around it down to e and up to b was also sensitive. The extent of this sensitive area and the more sensitive points in it fluctuated from time to time. There was also for a time a sensitive $c\sharp$ and d. Generally also tones lower than the lower end affect the low end of this area and higher tones the higher end[2]. This is the usual state of things and seems to occur when there is only one sensitive area with a most sensitive point.

It is important to notice that long-lasting tones, such as those of the violin and the flageolet, and also intense tones produce correct tones, while the brief tones of the piano produce pseudo-tones[3]. This bears out the idea of sensitive points; a full and proper stimulation is still able to affect the normal point.

(*b*) *Orthosymphony.* Here we have a suggestion for a theory of the phenomena called by von Liebermann and Révész 'orthosymphony,' *i.e.* that when two tones were sounded at once[4], the interval was always correctly judged. To get the correct interval, however, it was necessary to take the chord as a whole without analysis. If the observer listened to get the single tones, he heard them as pseudotones. von Liebermann and Révész therefore supposed at first that both pseudotones were then really given. But later they believed and argued that this was not so; for "in dem Augenblick, wo das Heraushören gelang, wurde die dem richtigen Intervall eigentümliche Konsonanz nicht im geringsten verändert; die Verschmelzungsstufe blieb dieselbe, hinsichtlich des Intervallurteiles trat aber Verwirrung ein, da die Vp. naturgemäss nicht im stande war, ein Urteil zu geben über ein Intervall, das bei der Zerlegung andere Komponenten lieferte, als nach dem Gesamteindruck zu erwarten war[1]." But these and all the other statements of the paper only show that the degree of fusion, not the pitches of the fusing tones, remained constant[2]. In fact everything points to the idea that in the *Gesamteindruck* other pitches were there than in the analysed impression or in the single tone. If one maintains that in the *Gesamteindruck* the false tones, though not heard, were really there, one might as well assert that when an intense tone or a continuous tone of the violin or flageolet evokes a correct tone, the pseudotone is really there, though it seems to be absent[3]. There can be no doubt that the pseudotone appeared in chords only when it was supported by the attention[4], but that synthetically observed chords and intense and continuous tones, unsupported by the attention, were heard true. The abnormal sensitivity of the ear suppressed the normal sensitivity only when the latter was unsupported or briefly stimulated[5].

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[1] L. and R. 1, 265. [2] L. and R. 2, 308 (table). [3] L. and R. 1, 264, 274.

[4] Or successively (arpeggios), *i.e.* physiologically and psychologically overlapping and therefore partially simultaneous, see L. and R. 2, 298. Also in melodies with accompaniments, *op. cit.* 299.

(*c*) *Theory of the preceding.* It is not true that according to Ewald's theory or to any other we should have to say: the total impression is independent of the form of the sound picture[6] or of the actual effect of the stimulus on the receiving bodies; but we should have to qualify these theories by an addition to the effect that the *pitch* of a tone does not conform solely to the conditions laid down by the stimulus, either for single tones or for chords; doubtless it bears, as *e.g.* in Helmholtz's theory[7], a specially close relation to the stimulative conditions; but this relation need not always be unmodifiable and final; the general expression, which includes it, will rather refer to the point of the basilar membrane that is stimulated most effectively, whether because of its own greater sensitivity, or because of the greater intensity of the stimulus, or because of the mutual support of several stimuli acting against the effects of greater sensitivity[1], or because of the heightened sensitivity that attention gives to an already abnormally sensitive point, acting against the effect of mutually supporting stimuli. These various forces, which sometimes concur and sometimes oppose one another, suffice to explain all occasional exceptions to orthosymphony, *e.g.* that melodies given with accompaniments are not affected by the attention[2], and others.

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[1] L. and R. 1, 270.

[2] Cf. L. and R. 2, 298.

[3] From a certain aspect of the theory which I shall later develop, both of these statements are true of the real components of tone, but for the present and from the phenomenal aspect both are surely false.

[4] Cf. L. and R. 1, 270. Pseudotone given first and attended to kept its pitch even when a second tone was sounded.

[5] Certain familiar facts of touch form a curious parallel to orthosymphony. In certain disturbances of the peripheral nerves of touch a single touch is very badly localised, whereas simultaneous points are discriminated from one another better than usual.

[6] L. and R. 1, 271.

[7] Helmholtz, *Sensations of Tone*, transl. A. J. Ellis, 3rd ed., London, 1895, 144, fig. 52. Cf. also the extensions suggested by A. A. Gray, *J. of Laryngology, Rhinol. and Otology*, 1905, xx. No. 6.

Such a modification retains for the theory of hearing what is for very many reasons absolutely indispensable, namely a basis for the modification of analysis by means of the attention[3]. The pitch of a tone thus depends on the point of the basilar membrane that is for any or all reasons most effectively stimulated. The fusion of tones and the varying volumes of them upon which fusion rests, must therefore depend on the extents of the basilar membrane that are stimulated[4]. It is perfectly consonant with this view that the occurrence of beats is dependent, as in normal hearing, on the objective pitch, *i.e.* on the way in which the basilar membrane is affected by the stimuli, apart from greater sensitivity, attention, etc.

On the other hand the theory of illusion proposed by von Liebermann and Révész is not acceptable. After all an illusion of presentation is only 'illusory' by comparison with the objective stimuli. We cannot suppose that such an illusion belies and covers phenomena as well as realities; for that is what the authors' explanation really means. The impression of correction, they think, is a phenomenon which displaces another phenomenon, which, however, is not phenomenal at the moment. If musical experience can have the associative effects here ascribed to it, why does von Liebermann not hear the correct (imaged) pitch along with the (sensed) pseudotone? We should expect as much from what we know of visual images. And if it be said that tonal images do not mix themselves into tonal sensations, then the whole explanation given by the authors is invalid. It is therefore best to suppose that in all cases the pitch heard is phenomenally given, not reproduced[1]. This view is supported by the fact that the presence of the pseudotone in a chord seems to depend as much on the intensity of the components as upon attention[2]. So also, of course, the other aspect of tone-volume, or, as von Liebermann and Révész call it, *Höhe*,—is phenomenally given, not reproduced, in spite of the fact that in this unusual case pitch and volume vary independently.

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[1] Orthosymphony seems to be better with strong tones. See L. and R. 2, 292, 310. Cf. what is noted above, that intense single tones give no pseudotone.

[2] L. and R. 2, 299.

[3] L. and R. report no gliding tone (*Gleitton*) at the moment in which the pseudotone is heard in the chord by the analytic attention. Nor should we then expect any; on my hypothesis no change takes place on the basilar membrane at this moment. But when the pseudotone changed during a single observation and a *Gleitton* was heard (see L. and R. 2, 306) we may suppose that the basilar membrane actually shifted its point of maximum yield to the stimulus.

[4] A theory of hearing which relies on the statement of the preceding sentence alone must appear to be arbitrary. Why should the effects of all the sub-maximally stimulated resonators be suppressed?

(*d*) *Pitch the more precise basis of judgment.* All the statements made by these writers about the different relations of pitch (quality) and of volume (*Höhe*) to absolute judgments in terms of tonal names and to judgments in terms of names of intervals may be accepted as important additions to our knowledge. Evidently the preciser basis of the arrangement of tones and of the naming of them and of their differences (intervals) is pitch; but differences of volume also offer a basis for these judgments, although it is less efficient and exact. This agrees entirely with my proposal to class pitch with local sign and position as a kind of 'order,' to class distances and tone intervals in one group of experiences and to refer them to a common foundation in differences in the attribute of order[3]. Our judgments regarding tones and intervals are best when based on this attribute of pitch. But even when the pitch is distorted so as to be unreliable, the same judgments can be got from the volumes of tones by abstraction from the order most prominent in that volume. This may be compared with the fact that we can compare lengths of line as such without any comparison or superposition of their points, although the latter method is much the more precise. We have then to suppose that the ear provides us with a single series of orders; pitch is judged by the most prominent order; interval by the 'form[4]' constituted by the prominent orders; volume by the line or mass of orders stimulated at all; pitches and intervals can therefore be compared and fixed to some extent by means of volumes alone.

(*e*) *von Liebermann's deep symphonic pseudotone.* The hypothesis I have indicated can also be applied to the facts observed by von Liebermann[5] relating to the raising of a deep tone, *e.g.* C_1 by a fifth when played after its octave C. The affected region extended from E_1 to B_1, for only within this area was displacement difficult or impossible[1]. There the objective stimulus and the abnormal sensitivity of the ear coincided. Or else the region B_2—$D_1\sharp$ was less sensitive than usual. What the physical cause of these peculiar changes of pitch was, it is not easy to imagine. But it is abundantly clear that it was not central, but peripheral. For the phenomenon was not subject to von Liebermann's choice: it was compulsory. The five reasons given in the text[2] for a central basis are: (1) that the true tone appeared on insistence upon it; (2) that the idea of a comparative C turned C_1 into G_1, although only C_1 was sounded; (3) that the disposition of C_1 to become G_1 varied from time to time; (4) that the occurrence of C_1 or G_1 sometimes depended upon the will and attention; and (5) that only the pitch of the tone was affected, not the intensity, timbre, or volume. But these reasons are not cogent. For in the main abnormality shown by this subject, which can hardly be supposed to be of 'central' origin, insistent attention also affected the phenomena in orthosymphony, and the disposition to abnormality changed. The second reason given may be supposed to be due to the effect of attention coming to the support of the greater sensitivity of the ear. Without the help of the attention the normal disposition for single tones is stronger than the abnormal[3]. The physical basis of the abnormality may be to some extent concerned with the effect of the momentary intermingling of the stimulations which takes place when a deep tone follows its octave. But it must remain obscure on any theory. As the phenomena are the opposite of orthosymphony, it might be thought to be due to blunting of one region, not to increase of sensitivity of the other—E_1—B_1.

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[1] "Der orthosymphonische Zusammenhang erscheint, wenn der Korrektionseindruck da ist, normal in jeder Beziehung," L. and R. 2, 299. Then it *is* normal.

[2] "Auch wenn der Zweiklang unmittelbar nach dem Anschlage orthosymphonisch erscheint, so kommt doch mit fortschreitendem Abklingen der Pseudoton immer mehr zur Geltung," L. and R. 2, 310.

[3] This *Journal*, 1911, IV. 143 ff., 172 ff., 179 f.

[4] Cf. below, 38 ff.

[5] L. and R. 3, 325 ff.

(*f*) *Binaural mixture.* The hypothesis accepted by me can also be extended to the facts of binaural mixture observed by these writers[4]. It is well known that the ears of many persons, if tested singly, are found to be of different pitch; for the same objective stimulus they render two tones of slightly different pitch. But when both ears are acting at once, no diplacusis occurs[5]. In von Liebermann this pitch difference occurred in a higher degree than usual. Each uniaural tone seemed to stand away from the binaural tone in opposite directions. If the uniaural tones were of equal intensity, the binaural tone was that tone which is normally evoked by the average of the numbers of vibrations which normally evoke such uniaural tones as were heard. If the uniaural tones were of different intensity, the pitch of the binaural tone approximated correspondingly to the pitch of the stronger tone[1]. Von Liebermann and Révész see in these facts evidence of a mixture of tonal qualities. The comparison is just, provided of course it is correct to classify pitches as tonal qualities. They take pains to explain why tonal mixtures within a single ear do not occur, viz. because uniaural tonal qualities occur in different 'heights.' But if this be a 'reason,' do we not then lose all touch with the analogy of vision? We do indeed; for at the next moment we read that 'heights' are (not, as it were, differences of brightness, as we read elsewhere[2], but) as it were corresponding points of the two ears; so that when the same 'height' occurs in both ears, the qualities mix. And if these observations are evidence of tonal mixture, then the resulting mid-tone must be supposed to resemble the two mixing tones. Then, as Révész suggests elsewhere, the whole variety of tonal 'qualities'—the scale between octaves—will also be reducible to two end qualities. But it has commonly been denied that *d* resembles *c* and *e*, as an orange-red resembles both red and orange.

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[1] *Op. cit.* 327.

[2] *Op. cit.* 331 f.

[3] *Op. cit.* 332.

[4] L. and R 4, 676 ff.

[5] Cf. Stumpf, *op. cit.* II. 320 ff.

Theory of its occurrence. A sufficient explanation of these binaural 'mixtures' can be found in a much simpler hypothesis. For it does not involve the admission of uniaural mixtures, and it demands neither the presence of phenomena which do not occur, nor the assumption of phenomenal illusions of presentation. It is agreed that the pitch of a tone is determined by the point of the basilar membrane that is most intensely stimulated. This maximum of stimulation is surrounded on both sides by a region of decreasing intensity of excitation. Now in binaural hearing, as the objective stimulus for both ears is the same, the length of the basilar membrane affected will remain the same, and therefore the volume of the tone in each ear will be the same. We have only to suppose, what is highly probable,—that all the points or pitches of the one basilar membrane are connected with those of the other physiologically, and the phenomena observed by von Liebermann and Révész will immediately follow. For then a superposition of the stimulations, *i.e.* of their intensities, would follow[3]. If the pitch displacement of the two ears is small (in von Liebermann's case it was never greater than a semitone[4]), the resultant order or pitch will naturally fall where there is the greatest resultant stimulation; as the intensity from the other ear increases, the maximum point will move over gradually to the pitch of the other ear. The physiological hypothesis here involved is already familiar in Bernstein's theory[1]. It is, in fact, merely an application of Bernstein's hypothesis to the facts of hearing. But it would not at all follow that a similar mixture should hold for uniaural tones. For there the superposition of intensities is affected by a number of other physical disturbances which give rise to such things as beats[2]. We suppose, that is to say, for uniaural tones that there are really two different stimuli, two rates of vibration, whereas in this binaural case the stimulus remains the same; only the way it affects each ear differs, because each ear differs. It is at least evident that the mixture theory of von Liebermann and Révész does not necessarily apply to the whole series of their qualities at all, and the analogy with vision must thus far break down. Besides their whole theory of the segregation of miscible qualities by means of the attached 'heights' must fall to the ground, unless they can show why 'quality' is segregated with 'height' at all. And the doing of that is excluded by the very use of the category of quality. Similarly Révész cannot show why in von Liebermann certain qualities have dropped out, while others remain, and why different stimuli can excite the sole remaining pitch. Will he recur, like Jaensch, to the assumption of differences of age amongst his qualities? The use of the category of quality blocks all advance here. Nor can Révész explain why certain qualities drop out only in certain octaves. If the lost qualities are really lost, they should be lost altogether.

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[1] Cf. also Révész, *Zur Grundlegung der Tonpsychologie*, 63. Stumpf noticed something like this, *op. cit.* II. 326 f.

[2] Révész, *Zur Grundlegung der Tonpsychologie*, 41 f.

[3] Cf. L. and R. 4, 680. The binaural tone is stronger than either uniaural tone.

[4] Révész, *Zur Grundlegung der Tonpsychologie*, 64.

In certain cases, where perhaps the pitch difference of the two ears is greater and where one ear has been mistuned in an irregular way[3], both tones are heard at once, as if they were presented to a single ear, except that no beats occur[1]. A well trained observer will then be able to attend to the tones of each ear separately. Without this attention the general effect will still be that of a great dissonance, since the superposition of the maxima of the two ears will produce a resultant containing either two maxima or a line of maximal stimulation, *i.e.* the effect produced in one ear when dissonances arise from the simultaneous occurrence of neighbouring tones. At the same time the volumes of the two tones will not quite coincide, whereas in orthosymphony they do, thus giving a degree of fusion that is not consistent with the pseudo-tonal components that appear upon attentive analysis. There should be rather narrow limits to the possibility of cases like that of von Liebermann, where there is an even passage from the maximum of one ear to that of the other. A change in the resonators of one ear, whereby they are lengthened or shortened and so respond maximally to lower or higher tones than usual, forms a sort of auditory squint.

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[1] Cf. in W. Nagel's *Handbuch der Physiol. des Menschen*, T. Thunberg, "Physiol. der Druck-, Temp.- u. Schmerzempf." 1905, 720 ff. Cf. A. A. Gray, *op. cit.*, who refers to the discrimination of tactual points for an analogy to the discrimination of tones on the single basilar membrane.

[2] But under certain circumstances a better mixture can be got than results from the simultaneous action of two neighbouring tones, viz. by 'mixing' many neighbouring tones. Cf. S. Baley, *Ztsch. f. Psychol.* 1913, LXVII. 271 ff. It need hardly be said that Baley's results do not favour Helmholtz's theory as against Ewald's or mine.

[3] I have recently had occasion to observe this in myself in degrees varying from a semi-tone of difference to a just noticeable deterioration of the timbre of sounds. In the former case the flat pseudotone is located in or opposite the left ear and it has an unpleasantly metallic timbre; melodies and voices of the appropriate pitch are heard double, but the false ones are devoid of any proper tonality. In the latter case tones that are usually full and round sound slightly flat and rather metallic.

(*g*) *Vocality.* Révész attempts to show that vocality is a third aspect of sound alongside quality and height[2]. His chief argument is the case of N. N., who could only hear as far as e^2, and yet could recognise the vowel *a* and even *e*, *i.e.* (according to Jaensch) average rates of vibration of 1000 (c^3) and 2000 (c^4) per sec. If this observation is correct as it stands, vocality would become not only an aspect of sound, but a component of it, a kind of subsense. The observation must therefore be treated with great reserve. Besides an average rate of vibration with a fair variation might very well be heard when no tone could be heard. In the same way, we hear vowels without being able to sing their pitch, especially when they are given on a fundamental tone, as the vowels were given to N. N. (on c^0). A vowel is a complex and irregular impression which might well be effective when a regular impression was ineffective.

(*h*) *Interval.* Révész has devoted a considerable part of his book —*Zur Grundlegung der Tonpsychologie*—to a discussion of interval. Interval, like the pitch of a tone, may be judged either promptly or by slow laborious effort. The former is based in both cases on differences of quality, or, as I call it, pitch; the other rests upon the vaguer *Höhe* or volume[3]. The former Révész calls interval, the latter distance.

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[1] Cf. Stumpf, *op. cit.* II. 460; L. and R. 1, 269, 274.

[2] *Zur Grundlegung der Tonpsychologie*, 84 ff.

[3] In connexion with this distinction Stumpf's note on the possibility of such a thing is of interest. *Op. cit.* II. 336.

There is, as he shows, a great difference between the intervals formed by successive tones and those formed by simultaneous tones. The latter are much harder to analyse; very often the tones have to be attended to successively. Differences of 'height,' *i.e.* distances, recede very much in chords. And even the pitches of tones and the 'intervals' based on them are not easy to hear then. Spatial symbolism, *e.g.* deeper, higher, is only applicable to successive intervals.

Further theoretical indications. These and other facts mentioned by Révész are immediately explicable on the hypotheses I would retain for the physiology of hearing. For if pitch depends on the more intensely stimulated region of the basilar membrane, it is obvious that two pitches together will immediately offer great difficulties, especially when both are given equally intensely. They will tend to suppress one another or to detract from one another's 'point.' Only if the attention is turned to them successively, supporting first one and making it more intense and then the other, will the analysis be easy[1]. Orthosymphony, even on the illusional theory given by von Liebermann and Révész, may itself be partly accounted for by this means, although the fact that the two tones cooperated and so stimulated the basilar membrane quite as they normally do, is of primary importance. In chords, moreover, the extents of the basilar membrane excited by the components of the total sound wave must often be largely coincident, so that there must be a large amount of identity amongst the orders of the elements of auditory sense of which they consist, and they will therefore in many cases fuse very well. The more exact the coincidence of the parts of the basilar membrane that are stimulated, the more will the maximal region of the higher tone add itself evenly and regularly to the maximal region of the lower, extending it and it may be giving it a slightly different form. Thus tones which fuse according to volume will tend to fuse according to pitch and two tones will then tend to be taken for one. Only when the maximal points of the fusing tones are close together, will they interfere with each other markedly and give a broad line of maximal stimulation with oscillations due to the differences in the number of vibrations per second. They will be heard as increasing dissonances, the nearer they are together and the nearer they come to an even superposition without reaching that entirely. Some chords will therefore be easy to analyse, others will be difficult. But whatever the ease or difficulty of analysis may be, each chord will be a characteristic complex of tonal orders and so will be recognisable as a unitary whole, or at least distinguishable from other complexes, whatever the stage may be at which the analytical training of the individual has reached. The same hypothesis offers an explanation for the fact that musical people often fail to notice the presence of difference-tones and overtones. They notice generally only what their attention is trained to pick out by the successive analysis of the attention. All the rest goes into the sound picture; it does not remain ineffective; it makes a difference to the total pitch picture; but it does not excite analysis until the attention is turned by special means towards the difference-tones and partials[1]. The facts of the separate discovery of difference-tones, combination-tones, and overtones is an obvious proof of this[2].

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[1] Cf. Stumpf's "Mit dem Ohre singen," *op. cit.* II. 291 ff.

This view also explains why in a complex tone or in a single chord the fundamental tone is the most easily apprehended. It forms the greatest extent of stimulation, its maximum is more widely spread, and also more intense as a maximum than that of any other tone in the chord, unless it be a much higher and very intense tone. When successive chords, however, are given as accompaniment to a melody, it is the melody which is noticed most easily, *i.e.* oftenest by musical and unmusical observers alike. For the melody is commonly the part that moves most connectedly and most effectively[3]. This motion draws the attention most to itself, just as it does in vision, where also of stationary points of light the most intense is most noticeable.

As regards the ambiguity of intervals formed by 'qualities' divorced from their usual 'height,' the facts recounted by Révész[4] only confirm the distinction between 'quality' (pitch) and 'height' (volume). Any and every theory of these facts has still to find a reason why the ambiguity of an interval given by hypothetically pure and unattached

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[1] Cf. Stumpf, *op. cit.* II. 232.

[2] It must be evident that since the fusion of tones is not a property of the elementary sensation, but only of complexes of two or more sensations, we must give a psychological theory of it first (cf. Stumpf, *op. cit.* II. 211 ff.) and use that to formulate a physiological basis for it. Both these kinds of theory, of course, will refer primarily to the corresponding properties of the most elementary auditory sensations we actually get. These, however, must be analysed into complexes of hypothetically ultimate elements of auditory sensation, which differ from those of other senses only in the matter of quality. The structure of the complexes which form our simplest auditory sensations, must explain all the special peculiarities of sound.

[3] Cf. Stumpf, *op. cit.* II. 337 ff.; also 393, "Bei aufeinanderfolgenden Zusammenklängen macht das Ganze scheinbar die Bewegung der in den grössten Schritten bewegten Stimme mit."

[4] *Zur Grundlegung der Tonpsychologie*, 113 ff.

'qualities,' *e.g.* *c*—*e*, was perhaps oftenest determined to that of a rising third, why it was sometimes unresolved and therefore arbitrary, and why it was sometimes determined to that of a falling minor sixth. Révész's facts nowhere prove the absolute independence of 'quality' and 'height,' but only the relative independence of them[1]. Nor can he raise more than a presumption in favour of identifying the 'qualities' of a tone and its octave; for in spite of their similarity and approximation to identity, there can be no doubt that the uppermost quality of an octave comes next after that of the tone that just precedes it, when we play up the scale; it does not leave the latter aside to begin afresh, as we should have to admit, if we suppose with Révész that the tone *c* and the tone just less than c^1 are maximal differences from which all intervening tones are probably derived by mixture. Besides, Révész's theory of the absolute independence and ambiguity of quality does not do justice to the fact that we can begin an octave on any quality whatsoever. Also if 'qualities' were miscible, we should for that reason have to suppose that the maximal differences within an octave were all those qualities which are separated by a little more than the tritone interval[2].

Moreover the facts suffice in no way to show, as Révész maintains[3], that an interval has a direction only because it is normally, *i.e.* habitually, associated with a difference of 'height.' Here we encounter again the misleading influence of the analogy with vision. If 'height' has direction, it must also include some basis of 'position'; by what means then is Révész going to show how his 'qualities' have got linked up normally with differences of position? And if he attributes some rudiment of position to his 'qualities' also, he will find it hard to show why there is only one range of them, while there is a much larger range of 'heights' and therefore of positional differences, capable of using up the octave over and over again. Of course there is a problem involved in the facts. But that problem is to explain the special differentiation of the ends of the series of pitches, not by any means to explain that the ends are distinguished at all and that there are two directions of change within the series. Positions and directions are distinguished in touch, vision, and articular sensation; the last of these forms the best analogy to sound, as it presents so often a single series. But in sound we call the one end high and the direction towards that end 'rising,' the other end low and falling. This special distinction, I take it, is one that could well be explained by the help of the normal accompaniment of differences of volume. For these latter differences form a series with obviously different ends, the one having first a physiological, and then a physical limit[1] of smallness, the other hardly a limit of largeness. The occasional ambiguity of the direction of intervals is more easily explained by the peculiar conflict they entered into in von Liebermann's case with the direction of volume-differences. Other cases may obviously be put down to the confusion of scale relations, *e.g.* thirds, tenths, etc. Besides, of course, Révész's appeal to habit belies the very kind of discovery he derives from von Liebermann's observations, viz. that the 'habit' of the normal correlation of 'quality' and 'height' can be dissolved and observed in dissolution.

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[1] Or, more strictly, of volume and the most pronounced 'order' within that volume.

[2] Cf. Stumpf, *op. cit.* II. 201 ff.; Révész, *Zur Grundlegung der Tonpsychologie*, 134.

[3] *Tonpsychologie*, 116 ff.

After all, the conviction must force itself upon one that the octave relationship is not to be explained by a presumption of identity based on the presence of great similarity, but by reference to the fundamental properties of all sensation in relation to the simplest complexes in which auditory sensations are given to us, *i.e.* by relation to coincidences of volume. It is only such a theory that can explain the absolute relativity of scale relationships, which is, nevertheless, accompanied throughout a large part of the auditory range by the discrimination of practically absolutely equal differences of pitch. The decrease of clearness of pitch at the lower end may be readily explained by the greater diffusion of the region of maximal stimulation, and the analogous nature of the upper limit by the difficulty the soft basilar membrane presents towards fine stimulatory differentiation as well as the want of a sufficient number of sensitive spots to receive the fine differences of position which successive tones imply. But as the volumes are extents, there might still be a fair differentiation of tones in that respect; although it is obvious that both pitch and volume differences must deteriorate together in the highest regions. Still as the extent has always a greater basis than pitch, the former might somewhat outlast the latter in high tones[2].

As for all the problems, "die gar nichts Problematisches haben, so lange man Intervall mit Distanz identifiziert," namely the problems of transposition and inversion, it must now be clear that they arise only for those who identify pitch with quality and volume with something quite heterogeneous—height. If the relation between pitch and volume I have suggested is adopted, it becomes clear at once that transposition and inversion and the like are only matters of form, which present no other problem than do any matters of form, *e.g.* those of vision and touch. We recognise a form more or less in any part of the visual field and more or less in any relation to the frontal plane, *e.g.* laid on its side, or turned upside down. And hearing offers us parallels to practically all the familiar processes of change of form, namely motion[1], speed and all their relations to emotional life as signs of the activity of experience in general.

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[1] Thus I dissent from the view of Révész and others that the *Höhenreihe* is 'prinzipiell unendlich,' *Zur Grundlegung der Tonpsychologie*, 87.

[2] Cf. Stumpf, *op. cit.* II. 57, "Wo man noch den bestimmten Eindruck hat, dass ein Ton spitzer ist als ein anderer, den man doch seiner Qualität nach nicht mehr davon unterscheiden würde."

V. *The nature of the system of hearing.*

The result of the preceding discussion of later researches on hearing is to confirm on all notable issues the formula I proposed for auditory sensation on the basis of a general study of the common attributes of sensation. I need not again proclaim any of the principles upon which I found a study of the senses that is both systematic and special. In fact they must occur to anyone who sets himself the task of such a study, confident that it must succeed.

We must therefore admit that the sense of hearing brings us only one quality, differences of intensity, of pitch (which falls under the generic head of 'order'), of volume, and of temporal attributes. These differences of volume introduce the problem of compound sensations and we solve that problem by postulating primary atoms of audition, which we practically never experience in isolation. We get them only in masses, in which a small region of orders is more or less definitely emphasized by means of intensity. From this psychological analysis and theory we can proceed to formulate the hypothetical nature of parallel physiological processes and to explain all the special phenomena of sound. Only known and commonly accepted physiological hypotheses need hereby be invoked. The psychological construction, on the contrary, is largely new; but that is no sign of its error, but only of the failure of previous efforts to find the correct line of analysis. Nothing else was wanting; for the psychological hypotheses involved in it are in other spheres of sensation the most trivial and familiar. No one could doubt that areal experiences of colour are obtained physiologically by the simultaneous stimulation of a large number of neighbouring sense-organs and psychologically by the fusion of a large number of elementary (*i.e.* smallest known to us) sensations of colour, differing similarly in order from one another and fusing in virtue of their extensity to an area. We have only to add the hypothesis that in hearing we never get sensations more elementary than such areas or extents. To turn from the clear daylight of such a view to the darkness of qualitative differences is to nourish a passion for ignorance and scepticism. On the qualitative line of analysis the time when we shall reach some understanding of the sense of hearing is indeed far distant.

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[1] Cf. This *Journal*, 1911, IV. 169 ff.

Only one point need again be emphasized[1]. The failure to appreciate it has been a common barrier to progress. Pitch and volume constantly urge their true nature upon our attention, but it has been called 'quasi-spatial' and so the truth has been ignored and suppressed[2]. But in fact far from being quasi-spatial, they are non-spatial. They are simply systemic, *i.e.* such orders and such continuousness as will with sufficient variation of order constitute a sensory system. The practically most important correlation of differences of systemic order in most senses happens to link them to the spatial differences of material things, both really and cognitively, both as a matter of fact and as a matter for our knowledge. So we call the tactual, the visual and other systems spatial. In the sense of hearing order differences are linked to spatial differences of matter only in fact; cognitively they are not so. But they form none the less a continuous system of positions. If we talk of this system as quasi-spatial, we should talk not only of the perceptual, but also of our conceptual systems as quasi-spatial. But that would be quite misleading. They are no more necessarily cognitively spatial than is any system of numbers. They are simply ordinal. Temporal differences of position are also ordinal. I distinguish this order from the other by calling the former temporal and the latter systemic[3].

VI. *Physiological theory of hearing.*

A physiological theory of hearing must fulfil three sets of requirements: (1) it must account for all the observed facts of hearing, and it must correspond adequately to the result of the general analysis and systematisation of auditory experiences; (2) it must be compatible with what is known or probable regarding the physical nature of the basilar membrane, etc.; (3) it must show how the sense of hearing has, or could have, developed, both physically and psychically, from its lower forms to the relatively advanced form in which we find it. Of the chief theories of hearing, Helmholtz's satisfies the first two of these conditions as well as might be, but as commonly understood, it does not satisfy the third at all. The general distribution of the functions of hearing over the basilar membrane which it assumes, appears to be the correct one, and it also correctly admits only one (at least only one most intensely stimulated) resonator for each tonal sensation. Ewald's theory, on the contrary, posits a whole row of similar physical processes for each single tonal sensation, and finds great difficulty in conceiving such connexions between the parts of the basilar membrane involved in the various sound-pictures and the 'centres' for each tone as will afford our actual awareness of tones. Ewald's theory is thus untrue to experience; we ought to hear for each sound picture a series of identical tones, but we do not. And it is as impossible to imagine how the connexions Ewald postulates, can have developed, as it is to imagine how a series of resonators can be placed in the ear and how the different 'qualities' (pitches) of tones came to be linked to them properly on the common psychological interpretation of Helmholtz's theory[1]. I wish now to indicate the kind of theory that would combine in it the advantages of the chief theories of hearing hitherto advanced and avoid the errors into which they have fallen. That is just what a theory that is guided by the correct lines of psychological analysis should be able to do. These lines have been found by the guidance of a properly planned analysis of the psychological facts of hearing and this again is inspired by a general consideration of the ways and means of attaining a complete systematisation of all sensory experience, at least of its simpler forms[2].

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[1] Cf. This *Journal*, 1911, IV. 142.

[2] Cf. Stumpf, *op. cit.* II. 55 ff., 58 f., etc.

[3] Cf. This *Journal*, 1913, VI. 241.

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[1] Cf. This *Journal*, 1913, VI. 254; also J. R. Ewald, *Arch. f. d. ges. Physiol.* 1899, LXXVI. 181 ff.

[2] Even so predominantly physiological a theory as that of Helmholtz has been able, as it ought, to suggest the question, whether "die Töne uns auch flächenhaft ausgebreitet und angeordnet im Bewusstsein erscheinen" (Stumpf, *op. cit.* II. 101). It is interesting to read Stumpf's note to these words, in which he reports Waitz's objections to supposing that tones are isolated by special organs in the ear. Stumpf says: "Derselbe Punkt schien mir früher bedenklich; doch könnte man, meinte ich, vielleicht noch die Hilfsannahme versuchen, dass die Töne zwar wirklich einen verschiedenen Ort in der Empfindung hätten, aber jeder immer denselben, wodurch der Tonraum ebenso bedeutungslos für unser Bewusstsein würde, als wenn er gar nicht existierte," etc. It can only be the confusing notion that *Ausbreitung* and *Anordnung* must always be spatial, instead of space being a kind of *Ausbreitung* and *Anordnung*, that thus forces Stumpf to make an hypothesis to conceal what is surely present.

As Helmholtz supposes, different parts of the basilar membrane subserve the reception of different tones; the higher whorls (towards the apex) of the cochlea give low tones and the tone evoked becomes the higher, the further we pass towards the basis of the cochlea and towards the root of the basilar membrane in the wall of the saccule. It is utterly inconceivable that, as Ewald's experiment suggests, the whole basilar membrane should be involved in the reception of every tone. Ewald's experiment is certainly very ingenious and instructive. But its main value is the proof it gives that the small fraction of his membrane[1] that forms the unit of any sound picture is the actual and sufficient physical response to an enormously greater wave-length. Any other value the experiment might have had, is annulled by its result —the sound picture—and by the fact that the artificial membrane used was straight and of equal breadth along its whole length[2]. Ewald seems to have felt it necessary to modify his theory to meet these considerations; for he suggests that under normal conditions the whole of the basilar membrane would be used only for low tones and that the higher a tone, the more it would confine itself to the narrower end of the membrane[3]. As regards the tone-picture, however few the number of repetitions it may contain, Ewald seems also to have felt the force of the psychological objection, that we do not for each rate of vibration hear a row of identical tones corresponding to the number of standing waves; for he sketches an hypothesis[4] which shall circumvent the necessity for this conclusion. But such an hypothesis is developmentally more impossible than is the correlation of a row of resonators with a row of pitches or 'qualities' on Helmholtz's view. Ewald himself points out this weakness in Helmholtz's theory[5].

The psychological analysis of hearing postulates not only that different parts of the basilar membrane shall subserve the reception of different tones, but that only one unitary part of it shall subserve the reception of each regular rate of vibration. This postulate is confirmed by the result of ter Kuile's investigation, who holds that for tones of different pitch a variable length of the basilar membrane is bulged out, beginning from the base and proceeding towards the apex of the cochlea. This view would give a good physiological basis for differences of volume, but it is hardly satisfactory in its explanation of pitch; for receptors that stand at the limiting points of the extent stimulated[1], cannot well be stimulated sufficiently for the purposes of the sharp determination of sensory differences. In sound, as in vision and touch, there will hardly be at the edges of a stimulated extent or area the most rapid change of stimulation, from positive to nothing, or from positive to negative; the change should be gradual in sound. For that reason it seems better to transfer the points of stimulation with Helmholtz to the middle of the extent of stimulation, which decreases in both directions towards the sacculus and towards the hamulus[2]. And if ter Kuile's conception, according to which any tone excites twice as long an extent of the basilar membrane[3] (always starting from its base at the saccule) as does the octave of that tone, could be combined with Helmholtz's, according to which a certain series of resonators is stimulated for any tone, the amplitude of resonance rising from *nil* at two points towards a maximum between them[4], it seems to me we should then have a perfectly sufficient basis for a full physical parallel to all the phenomena of sound. To the minimal complexes of auditory sensation which we experience, we should have corresponding minimal physical complexes. Multiplication of these complexes would give on both sides similar secondary features of tone. Fusion, especially, would receive an explanation on this view, as it gets on no other except ter Kuile's and Ewald's. In ter Kuile's theory, however, the task to be fulfilled by the explanation is overdone; for it becomes impossible to see why any but tones of great pitch differences should be distinguishable at all.

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[1] Cf. *Arch. f. d. ges. Physiol.* 1903, XCIII. 489 f.

[2] Cf. A. A. Gray, *op. cit.* p. 16 of offprint.

[3] *Op. cit.* 1899, LXXVI. 184 f.

[4] *Loc. cit.* LXXVI. 156 ff., 183 ff.

[5] *Loc. cit.* 155.

I have not yet found in the literature any explanation of the peculiar changes of curvature that are seen in the basilar membrane from its basis to the apex of the cochlea. These cannot be fortuitous, for they are almost exactly the same in all animals, no matter what degree of development their cochlea may present[5].

High tones would then be evoked by stimulation of the bases of the basilar membrane, while decrease of pitch would carry the extent of the basilar membrane stimulated further and further towards the apex of the cochlea. Lowest tones would probably affect the whole extent of the basilar membrane. But the region of the basilar membrane concerned in the determination of the pitch of the lowest tones would lie approximately at the centre of this extent. It would agree with this view that, as Ewald implies[1], dogs become deaf to lowest tones only when the upper end of the basilar membrane is considerably shortened. If the hearing of low tones is to be affected by mutilation of the basal end of the membrane, the whole of the first whorl must be destroyed[2]. It is a familiar fact that the differential threshold of pitch does not conform to Weber's law, any more than does the discrimination of points on the skin by the method of successive stimulation. The necessary 'increment' increases slowly with the rise of pitch. I have not been able to ascertain whether the nerve supply to the organs on the basilar membrane decreases from the basis towards the middle or is regularly distributed. Probably the silence of Retzius and other authorities on this point is in favour of the presumption of regularity of distribution. In any case we must suppose that the size of the differential increment of pitch is determined by the nature of the spread of excitation round the maximum and the influence of this upon the displacement necessary to give a noticeable difference. The displacement would be about the same relative amount throughout the scale, the same fraction of a vibration per second. Otherwise we should expect the discrimination of pitch to increase proportionately to the extent of the basilar membrane affected by a tone. Stumpf thinks[3] that the distance, as distinguished from the interval, between two tones increases up to the third accented octave. The nerve supply may possibly be richer near the bases of the cochlea than in the upper whorls; or our notion of the distance between tones may be affected by the number of differences we can distinguish between two bounding tones. Apart from the standardisation of distances which is given by consonance and by the relations of form between tones upon which consonance rests, it seems possible that we should take the finer subdivision we get in the higher octaves for greater distance. We do so in using the fingers or the tongue to appreciate distance; the former are to some extent standardised with other and less discriminating parts of the skin and with vision, but the tongue can hardly be said to be so.

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[1] *Arch. f. d. ges. Physiol.* 1900, LXXIX. 500 ff. It should also be noted that this limiting point is supposed to *move* forwards over the extent stimulated once in each tonal period. This oscillation of pitch is hardly reconcilable with the phenomena of hearing.

[2] It is quite consistent with this that the injury caused to the basilar membrane by a long continued intense tone affects not a point, but a short region of it.

[3] *Op. cit.* 201.

[4] This also agrees with Ewald's theory, in so far as a single unit of a tone picture is concerned.

[5] Cf. A. Gray, *The Labyrinth of Animals*, London, 1907, I.; 1908, II.

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[1] *Arch. f. d. ges. Physiol.* 1899, LXXVI. 179.

[2] It is quite possible that the remainder might after a time develop a response to high tones. Cf. Ewald, *loc. cit.*

[3] *Op. cit.* II. 405.

A physiological theory of hearing of this kind would easily satisfy all the demands of developmental theory. It would do this so far as the development of the receiving membrane is concerned, as easily as does Ewald's theory, and it would do so for the relations of the receiving membrane to the sensory centre, as Ewald's never could. For no new and inexplicable attribute of auditory sensation enters to upset the derivation it makes possible. We need only postulate a membrane of texture and length just sufficient to receive auditory stimulations, no matter how imperfectly, and connected with this a primitive auditory receptor, evoking a sensation of a certain quality[1] of variable intensity, having a certain 'order' aspect and extensity. A multiplication of these elements and an accompanying extension of the membrane will mean a variation of 'order' and a fusion of these orders in virtue of their common extensity or continuitiveness. This multiplication is already admitted in other senses and its biological advantage would be immediately patent in the modification of experience which would result from it alone. The sense of tone would, therefore, as Stumpf suggests[2], develop from above (high pitches) downwards (to lower pitches). Hand in hand with this development by multiplication would go a development by refinement of texture of the membrane and of the receptors, so that the experience would gradually approximate to that of pure tone, as the lengths of the radial fibres, etc., were systematically adjusted to one another in virtue of the advantages which variation towards that system would make patent through experience. The whole development can be seen at a glance from this point of view[3]. And there is not the least disparity between the peripheral and the central processes of complication or between both of these and the complication of the accompanying experience.

Another advantage of this theory is the ready explanation it offers of the *general nature of hearing*; complex sounds of all kinds first appear blended into a unity[4], which may afterwards be analysed. But this analysis never approximates to the kind of separation we find between different patches of colour in the visual field. That we can only explain by supposing that the cause of fusion is physiological and is not removable[1]. I suppose that the extent of the basilar membrane stimulated by every tone begins at the base near the oval window and extends towards the apex of the cochlea. The extent of basilar membrane involved by a higher tone will therefore always coincide with a part of that involved by a lower tone. Only in the case of a large difference of pitch will the maximal point of the higher tone stand well away from that of the lower and so be readily noticeable.

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[1] Ewald admits that on his theory there is only one "specific energy in Mach's sense" in hearing. *Arch. f. d. ges. Physiol.* 1899, LXXVI. 181.

[2] *Op. cit.* I. 339 ff., II. 218.

[3] For indications of the process of standardisation, whereby intervals, including the octave, are standardised throughout a large part of the musical scale and whereby pitches come to possess an 'absolute' identity and name, see below, 40 ff.

[4] Cf. Stumpf, *op. cit.* II. 77.

We should expect to be able to deduce the relative degrees of *fusion* shown in different intervals from a consideration of the nature of the supposed coincidence of extents. Thus it is evident that the extent involved by the octave of a tone will be the part reaching from the base of the basilar membrane to the maximal point of the lower tone. In the interval of the fifth the maximal point of stimulation of the higher tone and the end-point of the extent of the basilar membrane it involves will severally lie on either side of the maximal point of the lower tone and away from it by one-sixth of the whole extent involved by this lower tone. The corresponding relations for the fourth are one-eighth towards the base, one-quarter towards the apex. The next degree of fusion applies, according to Stumpf[2], to four intervals (4 : 5, 5 : 6, 3 : 5, 5 : 8); the corresponding relations for these are: one-tenth and three-tenths, one-twelfth and one-third, one-fifth and one-tenth, three-sixteenths and one-eighth. The relations for the tritone are three-fourteenths and one-fourteenth, and for the second and seventh one-eighteenth and seven-eighteenths, seven-thirtieths and one-thirtieth. Thus we see that only for the octave and the fifth is there complete balance in relation to the maximal point of the lower tone. For the fourth the balance is uneven, but it is only as two to one, and the denominators are still small. For all other intervals the denominators are larger and the balance is usually less. These facts seem to claim some significance alongside the oscillations of the stimulations which must accompany dissonances.

Beyond the interval of the octave, however, these relations must cease to be of much significance. And there is no doubt that fusion is then less[3], although the same harmonic affinities are maintained by indirect means. I mean by that, not overtones or other secondary accompaniments of chords, but the system of tonality that has grown up on the basis of the greatest and primary fusions.

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[1] *Op. cit.* II. 211 ff.

[2] *Op. cit.* II. 135; cf. W. Kemp, *Arch. f. d. ges. Psychol.* 1913, XXIX. 162 f.

[3] Cf. Kemp, *op. cit.* 162 f.

It is most natural that the grades of *sensuous pleasantness* of intervals should differ greatly from their grades of fusion[1]. For the former we have to appeal to other features of the tonal complex than its fusional aspect, namely its variety, a certain tension of parts not reaching as far as discord and the suitable footing for subjective activity that arises therefrom. But I shall not attempt to construct the physiological or psychological basis of all the phenomena of fusion. That is a task for special investigation. I wish merely to indicate the lines my general analysis would extend to meet the facts. I can see in the facts nothing which would constitute a serious objection to my analysis. On the contrary the facts rather seem to invite the application of it.

The relation of *attention* to analysis also receives a place in my theory. As Stumpf says: "Several simultaneous tones can be sensed and rough differences between them can be noticed without ado; finer differences only after practice and other favourable circumstances[2]." The aid given to attention by sounding one or more of the components of a chord before the whole chord, consists in the fact that the attention is thereby directed to that particular point in the tonal order which is to appear in the chord; the tone heard is thereby intensified, for its entrance is made easier and quicker. As the attention passes from one component to another, each is intensified and heard specially[3]. But we can only thus intensify what is already a relative maximum in relation to the just surrounding degrees of excitation. We cannot, as Stumpf[4] thinks we should be able to do on Helmholtz's view regarding the spread of sympathetic resonance on the basilar membrane, intensify by the attention any less than maximal points that are in no proper sense relative maxima. At the same time this does not prejudice the possibility that the pitch of low tones, for which the relative increase of resonance up to the maximum is very gradual, should be rather indeterminate.

The attention cannot without effort attend to all the differences that are given[5]; it must be trained, just as for the perception of the niceties of visual form; and it can be trained in different directions,—for analysis of the objective components of chords, for overtones and difference-tones, for accompanying significant noises, etc.; and the effect of imperfect training may fade out in time. Yet whatever the degree of training may be, all given differences are there and may be familiar in bulk without any analysis. Thus persons who make no analysis of sounds so as to single out their components one by one, may still recognise voices, noises, vowels, etc., simply by a comparison and distinction of these sounds in bulk. Limits may be set to analysis in any person by the nature of his basilar membranes; only a fine membrane will make very pure tones and a high power of analysis possible; in a coarser membrane total sound complexes, though tending in character more towards noise, will still be distinguishable and a certain amount of analysis will be easily attained. For noises are less constant and precise stimulations. So the powers of the musical and of the unmusical are all of the same nature and origin. Unmusical people are compelled to recognise sounds by a 'hear and say' method, as it were; those who have absolute ear may recognise most chords also in this way, while they will still spell out unfamiliar ones.

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[1] *Op. cit.* 192 f., 249 ff.

[2] *Op. cit.* II. 85.

[3] Cf. Stumpf, *op. cit.* II. 314, "dass mit dem gleichzeitigen Heraushören (zweier Obertöne) nicht, wie mit dem einzelner Obertöne eine Verstärkung verbunden ist. Verstärken kann man immer nur einen auf einmal."

[4] *Op. cit.* II. 113 f.

[5] For conditions affecting analysis see Stumpf, *op. cit.* II. 328 ff.

The greater ease with which sounds rich in *overtones* can be recognised[1] is just an instance of the greater ease of recognition of the complex as against the simple whole; it is also acquired on the 'hear and say' method. It is not through making the fundamental more precise that the overtones make the fixation of the pitch of the fundamental more precise. But the whole sound complex is made more precise and more easily recognisable by the addition of overtones. For the number of distinguishable differences in an octave increases greatly, as we pass from one octave to the next higher (up to a certain point); these finer differences must, therefore, react through the sound complex as a whole upon the distinction of that complex, when it is treated, as it usually is, as equivalent to the fundamental tone it contains.

But analysis can be carried only up to a certain point; it can never separate a tone from a complex in which it is given, so as to make it in all respects appear exactly as it does when it is given alone. Only in the matter of pitch, apart from quality and the temporal attributes which may be neglected, is there true equivalence. The pitch of the component, except perhaps in the most special cases[2], is identical with that of the isolated tone. The seeming perfection of tonal analysis rests entirely upon this fact. Intensity and volume, on the contrary, must be very much affected. There can be no true analysis of the intensity[3] of a component, except on the basis of the difference between the maximum, to which it owes its distinction, and the surrounding level, in so far as that is more or less appreciable. Analysis of volumes, such as would give the definition of volume found in the isolated tone, is also impossible. Component volumes can be detected only unitarily and, as we have supposed, through the secondary aspect of fusion. This is a general effect; analysis may be necessary to ascertain the nature of particular fusions in a complex chord; but we do not first, or at all, separate x and y from one another in respect of volume in order to notice their fusion. We attend to the x—y part of the complex in order to attend best to the x—y fusion, and that may be helped, of course, by first attending to the pitches x and y, so as to direct attention better to the x—y part of the fusion[1]. It is also obvious that in the process of analysis the unity of the timbre of a component will be largely sacrificed; the component will be heard without its timbre, unless special circumstances, such as the unitary movement of a timbre provided by the concert of several different musical instruments, make it specially easy to attend to a unit of timbre[2].

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[1] Cf. Stumpf, *op. cit.* II. 351.

[2] *Op. cit.* 397 ff. These, however, are not deemed true cases of change of pitch.

[3] Cf. Stumpf, *op. cit.* II. 420, "Die gleichzeitigen Tonempfindungen oder besser die gleichzeitigen Erregungen des Nervus acusticus tun sich gegenseitig einen Abbruch."

Special conditions will attach to the way in which either of two tones covers or obliterates the other, when both are given together. Stumpf's conclusion[3] "that the higher tone must possess a greater excess of intensity if it is to cover the lower one than conversely," seems to be deducible from consideration of the extents of the basilar membrane involved. For the extent of the higher tone will always fall within that of the lower, so that the amplitude of oscillation it produces will add itself to that of the lower tone. If the higher is to suppress the lower its maximum must lie so near to that of the lower and its intensity must be so strong that, when it is added to that of the lower, the relative increase of the maximum of the latter over its neighbours will no longer be enough to give a noticeable difference. The lower tone, on the contrary, has the advantage of being the only clearly defined one; it begins at the base of the basilar membrane and ends clear of all the higher tones. The others only help to colour it, to modify its form, so that it naturally usurps to itself the chief attention. If a higher tone is to be heard, it must not only make a bigger relative maximum (building already on a considerable amplitude) than it would in isolation; but it must be strong enough to call the attention away from the lower tone. This predominance of the lower tone in unchanging tonal complexes is probably the natural basis of Külpe's law of consonance, "dass bei gleichen Verschmelzungsgrade der einen Akkord zusammensetzenden Intervalle der grössere Verschmelzungsgrad des am tiefsten liegenden Intervalls für die höhere Verschmelzung des Ganzen den Ausschlag giebt[1]." That the special fusional relations of tones will modify these general rules is obvious. But fusional relations, as I have supposed, are to be considered merely as special cases of these rules[2]. "In a resting chord the whole seems to have the pitch of the deepest tone, even when this is not also the strongest[3]."

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[1] Cf. Kemp, *op. cit.* 214 ff., Summary, 235.

[2] For observations of the condition of tones in more or less stationary complexes, cf. W. Köhler, *Ztsch. f. Psychol.* 1913, LXIV. 100 ff.

[3] *Op. cit.* II. 228, cf. also from 219 onwards.

An important deduction regarding the facts of pathological hearing can also be made. The excitation of the basilar membrane that is produced by a tone that is the octave of another tone, just reaches the point of maximal stimulation of the latter tone. Any tone higher than the octave will not reach this maximal point and so will not include it within its extent. On the other hand the excitation of every tone lower than a given tone will include the maximal point of the latter within its extent. Now we may suppose that every diplacusis or pseudotone that is due to an affection of the basilar membrane arises because within the total extent excited by a tone there is, besides the point of maximal objective stimulation which gives the pitch of the objective tone, a sensitive point that is for some reason more sensitive than usual and so gives a pseudotone. It would then follow that the objective tone may be, according to circumstances and cases, any tone that is lower than the pseudotone, but never more than an octave higher than the pseudotone, if indeed it ever quite reaches the octave[4].

It is now easy to apply this theory of hearing to the exceptional facts gathered by von Liebermann and Révész. In a certain sense there is contained in any normal tone every pseudotone that can ever take its place, so long as the physical stimulus remains the same[5]. Pseudotones, it is clear, may be caused by any such alteration of the basilar membrane as changes the point of maximal stimulation, whether this process represent an increase of sensitivity somewhere, or a loss of sensitivity somewhere, or both, or a more or less regular change in the physical nature of the resonators of the basilar membrane. The pseudo-pitch is not really, but merely phenomenally, new; it is always contained in the normal tone, but it is then strictly subordinated to the normally most prominent pitch, *each tone being a regular system of sounds.* Noises are also sounds, but they are not the kind of system of sound that constitutes tone; they are not regular in system, but irregular; the irregularity may be of many kinds, just as noises are. We can hardly expect to be able to classify noises in any exhaustive manner. My analysis shows that it is scarcely a matter of great interest to do so. Theoretical interest centres chiefly on the question of the pitch of noises. Their pitch, however definite it be, will always differ from that of a tone by reason of the absence of the tonal system in which the pitch stands. But the very irregularity that surrounds the pitch must help to hide it. It will not predominate in a system of pitches, but it will merely be one of many simultaneous, or very rapidly changing pitches. Some degree of determination of the bulk of these will always be possible; we shall hear whether it is a low or a high sound, and in each case approximately how low or high. No more is surely needed. We do not ask the theorist who discusses the localisation of visual sensation to find the local sign of the sunbeam dancing on the water.

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[1] See Kemp, *op. cit.* 207.

[2] Cf. Stumpf, *op. cit.* II. 234.

[3] Stumpf, *op. cit.* II. 384, 407.

[4] According to Stumpf, *op. cit.* I. 277, mistuning in diplacusis to more than one-third probably rests upon illusion.

[5] Cf. above, p. 15.

With regard to the appreciation of intervals, the recent results obtained by Catharina v. Maltzew[1] are of importance. She has shown (1) that the judgment of successive intervals is not based upon their conversion into fused simultaneous intervals; the judgment is not based upon that form of the fusional relationships of tones. Nor is it (2) based upon 'distance'; for, as Stumpf observed, distance and interval do not vary together[2]; descending intervals are harder to judge than ascending, whereas both are alike in the matter of distance; and the introspections of Frl. v. Maltzew's observers show that the 'distance' between tones is a very unreliable basis of judgment, unless perhaps for grosser differences. She points out further (3) that 'distances' are gradually variable quantities, whereas interval varies qualitatively. The fact that in successive intervals we pass from one tone to another, leads her to recognise a special qualitatively peculiar experience of 'passage' in every interval and to assume that 'this content' it is that makes us speak of 'one and the same interval' (the pair of tones being of a certain relative frequency)[3].

These three points call for some comment. (1) It may readily be admitted that fusion in the sense of the simultaneous representation of the successively presented tones had nothing to do with the judgments given by the observers. But it has not been shown that such relations of tones as account for their fusion when simultaneous, do not form the ultimate means of the standardisation of 'distances' that converts them into 'intervals[1].' (2) It may also be admitted that intervals in this strict sense are not judged by reference to 'distances,' and yet it has not been shown that distances are not always involved and included in intervals. Intervals are distances that have somehow been standardised. (3) The time is past when qualitative differences can be established by a mere assertion, unless we are to use the term 'qualitative' in the loose sense of 'distinctive' or 'peculiar.' The bare assertion, therefore, carries no weight and may be dismissed. Moreover, on any theory, some explanation of the relation between 'distance' and 'interval' is called for, and this explanation will do much to settle what the psychological nature of the essential experience of interval is. I should not hesitate to assert that a proper classification of any 'peculiar' experience can only be given with the help of a systematic theory of the relations and connexions of the members of the large group of experiences to which it belongs.

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[1] "Das Erkennen sukzessiv gegebener musikalischer Intervalle in den äusseren Tonregionen," *Ztsch. f. Psychol.* 1913, LXIV. 161 ff.

[2] Cf. above, p. 31 f.

[3] *Op. cit.* 197.

I am not acquainted with any analysis, except my own, which suggests a ready explanation of the distance between tones, their interval, and the relation between these two things. Tonal distance, as I take it, is the parallel of visual or tactual distance. It is the crude, primitive 'mode,' the first and simplest of those that are founded on ordinal differences. Interval is, in general, a 'musical' term, a notion of the developed, discriminating, systematized, tonal consciousness[2]. Its nature is best defined by its simplest relation to distance; it is in the first place distance standardised. I do not mean to say that it is recognised or named by reference to distance, or that a judgment of interval is up to some point *eo ipso* a judgment based upon tonal distances; but it is a judgment which ultimately affects or concretes distances by standardising them. Frl. v. Maltzew's theory at the most merely points more correctly to the direction in which we shall find the means by which this standardisation is accomplished. It must be procured by the identification of something that is identical in intervals in spite of the differences of distance. She asserts—and I think it should now be clear that her view is merely assumption or assertion—that the basis of identification is the identity of the 'step' or 'passage.' But she does not show why the step from x to y is different from that from y to x. She does nothing to explain by reference to the familiar facts of other senses what the step or passage from one tone to another is. We learn much from her as to the connexions of similarity and familiarity amongst intervals and as to their relation to the memory. But that does not illuminate the problem as to their nature.

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[1] Cf. above, p. 31 f.

[2] Cf. This *Journal*, 1911, IV. 180, "In talking of interval in the primitive sense we cannot mean consonant, dissonant or 'tonal' intervals."

My analysis can only welcome, and be welcomed by, the results of Frl. v. Maltzew's careful study. It would point to the identical relations of form constituted by successive intervals as the basis of the standardisation of distances, and therefore as the true constituents of intervals. Simultaneous intervals are also marked by peculiarities of form, which, though they differ from those of successive intervals, are derived from one and the same source. I have already pointed to various reasons which will make simultaneous interval a very different matter from successive interval. The greatest difference lies in the fact that in simultaneous interval two 'systems' of sound are summed to make a new system, with a definite form, containing two special maxima, various less prominent maxima due to difference tones, etc., and in the case of many dissonances various other oscillating features. These features appear in successive intervals at most only for an instant at the onset of the second tone, if the time interval between the two tones is less than a certain amount, and they must be much blurred and obscured. Successive interval must be marked, as Frl. v. Maltzew assumes, by the special features of a passage from one system of sound to another. But here each system is given separately. The form of the whole unity of the two systems of sound is therefore not that of a combined system, but of a succession. I do not see how on any theory this complex of form (successive) could be turned into the other combined (simultaneous) unit by means of representation, unless the other simultaneous complex had been identified as containing the pitches given in the successive form, and the two had been correlated and were reproducible through memory. We might as well expect the untrained eye to draw the sum of two sine curves without calculation. Confusion of thought about the relation between simultaneous and successive intervals has resulted from want of clearness regarding the voluminous nature of sounds and the nature of the analysis of sounds made by the ear. As I have shown above (page 36) by reference to observation and by deduction from my analysis, only pitches can be analysed perfectly in simultaneous intervals, whereas intensities and volumes can only under special circumstances be separately gauged. But in successive intervals pitch, intensity, and volume all stand out clearly and separately in the successive 'systems' of sounds. The difficulty of holding, of recalling, and of singing certain of these intervals is the difficulty of passing from one sound system to another; it is clear that the presentation of the interval must largely determine the standpoint of the observer within the total combined system. The inversion of an interval means a great change of form unity. Identity of interval means identity of the form unity which the two successive systems form.

This unity may, of course, contain various secondary features which might serve as a basis for its recognition. Frl. v. Maltzew does not refer to any of them; they may, indeed, be wanting in any number of cases. She has shown the various means used by her observers to retain an interval and to make it clearer for recognition, such as transference to lower octaves, movements of the larynx, etc. All these aids presuppose the recognition of the peculiar form unity of successive interval. Only in the case of inference from recognition of absolute pitches is the form unity ignored. Direct and immediate estimation of interval[1] is the product of much previous memory work: correct classification presupposes processes of comparison of a more or less direct kind, which, as Frl. v. Maltzew has shown, still come into operation where intervals have to be judged in unfamiliar regions of sound. At the extremes of the tonal scale the form unities of intervals become very large and very small, and only by reference to the distortion of forms which for obscure physiological reasons then ensues, can we expect to explain the slight falsification of pitch which is found in the extremes of the tonal scale[2]. This normal false hearing does not necessarily imply a falsification of auditory distances, but only in the first place a disturbance of the usual processes for the standardisation of intervals and for the naming of tones which thereby becomes possible.

If both successive and simultaneous intervals involve attention to matters of tonal form, it becomes clear that tonality is the highest expression of the maintenance of continuity of form. Some basis for continuity of attention must be given, if complex sequences of sounds are to be apprehended rapidly. The more complex the nature of simple intervals is shown to be, the more necessary does this support to the attention appear. It may indeed be true that unity and proportion of form are made more noticeable by the increase of the complexity of the masses of sounds and the tonal forms they create; just as the unity and proportion of visual schemes are more effective in the larger visual works of art than in the simple parallelograms, triangles, and crosses of experimental analysis. But even so the attention of the observer must be guided in both of these arts and the more so perhaps in the successive structures of music. Tonality, or the introduction of a general scheme or system of intervals, needs no further justification in general than this. Only when we turn to the details of the schemes of tonality we actually find before us, do we need to enquire into the particular causes that have led to the formation of each particular scheme. The outlook that must guide these enquiries is clearly indicated in the psychological analysis and theory of hearing I have advocated.

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[1] As also of pitch, of course, for the naming of pitches must be a result of the standardising of intervals. Thus is the primitive ordinal difference between tones converted into a musical system. It must now be clear how superfluous such an hypothesis as that of the 'counting cell' (*Zählzelle*) is. Musical evolution, moreover, does not require the assumption of any rapid development of the sense-organ of hearing, but only the construction of varied and thorough systems, based upon such standardisation, of greater and greater complexity.

[2] Cf. Maltzew, *op. cit.* 216 ff.

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THE ACCURACY OF THE $\Phi(\gamma)$ PROCESS.

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1. *The probable error as sole criterion of accuracy.*
2. *Urban's examination of four processes.*
3. *Description of the* $\Phi(\gamma)$ *process.*
4. *Urban's calculation of its probable error.*
5. *Corrected formulae for the probable error.*
6. *A check furnished by subdividing the data.*
7. *Conclusions. Note.*

WHEN an experimenter who desires to measure a threshold has collected his data, and wishes to calculate the threshold, he must choose one of the various processes of calculation which are available. The process of calculation is quite distinct from the method of experimenting although the latter may influence the choice of the former, for the data given by some methods are in such a form as to make certain processes either impossible or inconvenient: and *vice versa* the desire to use a certain process may influence the choice of method. The available processes may be classified[1] into calculations (A) from the observed values or (B) from a smooth curve to which the observations have been adjusted. In this paper the accuracy of one of the latter processes is investigated.

1. *The Probable Error as Sole Criterion of Accuracy.*

It is important to notice that the adoption of a process of calculation really means the adoption of a definition of the threshold. Apart from special cases[2] we desire to find that point where the probability of the subject's reacting in a certain way (say answering *heavier*) is exactly one-half, and the question arises whether the definition of the threshold which we adopt by the act of choosing a process of calculation, does or does not closely agree with this point. But as we do not know this point except through the data and by means of the processes of calculation, the only criterion we can apply, and the criterion which in fact we are compelled to accept and do accept not only here but in all branches of quantitative knowledge, is a criterion based on the probable error. There exist processes which do not even theoretically pretend to measure the above point. This paper is however not concerned with such processes, which the writer hopes to discuss in future articles. The whole epistemological question of the degree of reality to be attributed to the threshold, and whether it is variable or fixed, is thereby raised, and on the practical side it must be decided whether the mean, mode, or median is its best measure. Without prejudice to any future comparison of these three the writer is here concerned only with comparing among themselves processes which under ideal conditions give the median or point where the probability is one-half. That process of calculating this point gives the truest value which gives the least probable error.

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[1] After W. Brown, *Mental Measurement*, Cambridge, 1911, p. 23.

[2] See for example G. H. Thomson, "The Best Form of the Method of Serial Groups," This *Journal*, 1913, v. pp. 398–416.

2. *Urban's Examination of Four Processes.*

Recognising this fact, Professor F. M. Urban, in an article published in 1909[1], examined as to their probable errors four different processes, two under (A) which are applied directly to the data, and two under (B) which assume a psychometric function, viz.:

A. 1. The Limiting Process, either direct or by means of Urban's Formula[2] $T = \Sigma Pr$.
A. 2. The Lagrange Interpolation-formula.
B. 1. The $\Phi(\gamma)$ hypothesis, assuming 'normal' distribution[3].
B. 2. The $\tan^{-1}$ (Arctan) hypothesis.

Professor Urban shows by certain means which need not detain us here that the Arctan hypothesis is less accurate than the $\Phi(\gamma)$ hypothesis. He then compares the latter with the two A processes by calculating the probable errors of each. His final results in this connexion are given in a table on page 415 of the work quoted, where probable errors according to all three processes are given. According to that table the Lagrange Interpolation-formula is very much more accurate than the Limiting Process, while the Limiting Process is a good deal more accurate than the $\Phi(\gamma)$ hypothesis.

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[1] F. M. Urban, "Die psychophysischen Massmethoden," *Archiv f. d. ges. Psychol.* 1909, xv. and xvi. pp. 261–415.

[2] Urban, *op. cit.* p. 291, etc.; see also Brown, *op. cit.* p. 32.

[3] Urban, *op. cit.* p. 366; see also Brown, *op. cit.* p. 25.

The present writer, however, has already shown that[1] Professor Urban's calculation of the probable errors in the Limiting Process is incorrect. This process is much more accurate than the table referred to in Professor Urban's article would lead one to expect. Indeed the probable errors given in the double middle column of that table ought really to be of the same order of magnitude as the numbers given in the first column for the Lagrange Interpolation-formula, and only an accurate re-calculation according to the corrected formulae would enable judgment to be passed. Professor Urban, in correspondence with the present writer, has expressed his agreement with the corrections and his intention of re-calculating the whole. Probably the Lagrange Interpolation-formula will prove to be slightly the more accurate of these two processes.

In the same correspondence Professor Urban also expressed his suspicions (which agreed with the writer's previous opinion) that some error lurked in his calculation of the probable error of the $\Phi(\gamma)$ process, and his hope that this might be discovered; and it is the object of the present paper to point out this mistake. The probable errors given in the last column of the table referred to, for the $\Phi(\gamma)$ hypothesis, are also very much too large. The cause and extent of the mistake are indicated in what follows, and the correct procedure pointed out. It is necessary to begin by describing briefly the $\Phi(\gamma)$ process, and Professor Urban's experiments.

3. *Description of the* $\Phi(\gamma)$ *Process.*

The experiments were on the difference threshold for lifted weights. The standard weight was 100 grams and was lifted before each of the seven comparison weights. The judgments given were *lighter* than, *equal* to or *heavier* than the standard. With Subject I the answers *heavier* were distributed as follows, in 450 trials with each weight:

Comparison weight r...	84	88	92	96	100	104	108
Answers heavier	1	9	40	100	186	403	423
Proportion p	·0022	·0200	·0889	·2222	·4133	·8956	·9400

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[1] G. H. Thomson, "The Probable Error of Urban's Formula," This *Journal*, 1913, v. pp. 217–222.

The third row of this table is simply the second row divided by 450. The assumption is now made that the relationship between r and p is

$$p - \tfrac{1}{2} - \frac{1}{\sqrt{\pi}} \int_0^{h(r-a)} e^{-x^2} \, dx = 0 \quad \text{..................(1)},$$

where a is the required threshold and h is its 'precision.' In passing it may be noted that later in his paper Professor Urban uses S instead of a for the threshold and x instead of r for the comparison weights. We shall use a and r throughout. By inserting in equation (1) the corresponding values of p and r from the above table we obtain seven equations for two unknowns h and a, and as these equations are slightly inconsistent with one another we have to decide how to calculate the most probable values of h and a. No pair of values will exactly satisfy all seven equations: instead of coming to zero they leave small residuals v_1.

Before we can proceed further we must note (as Professor Urban first pointed out), that these equations are not of equal weight, even although each is based on the same number, 450, of experiments. The weight of each equation may be taken as

$$\frac{1}{4p(1-p)} = \frac{1}{4pq} = B \quad \text{........................(2)},$$

based on an application of Bernoulli's Theorem, which assures us that p is most difficult to measure when it is near the value one-half.

We can now return to our seven inconsistent equations and their residuals v_1. If we assume—and by applying Bernoulli's Theorem we have in fact already assumed—that the errors of each p follow a normal or Gaussian distribution, that is to say if a large number of experimental values of the same p are grouped about their central value in a Gaussian fashion, then we can find the most probable values of h and a. They will be those values which make

$$\Sigma B v_1^2 = \text{a minimum} \quad \text{........................(3)}.$$

The conditions that this should be so are

$$\frac{d}{dh} \Sigma B v_1^2 = 0 \ \text{ for constant } a \quad \text{..................(4)},$$

and

$$\frac{d}{da} \Sigma B v_1^2 = 0 \ \text{ for constant } h \quad \text{..................(5)},$$

and these two equations, called Normal Equations, enable us to calculate h and a. Or rather they would do so if the original equations like (1) were linear: and here they are far from being linear.

To avoid this difficulty we look up in tables of the above integral (Fechner's Fundamental Table) those seven values of

$$\gamma = h(r - a) \qquad \text{.............................}(6)$$

which correspond exactly to our seven values of p. These equations are not yet linear in h and a but if we write

$$c = ah$$

they become

$$\gamma - hr + c = 0 \qquad \text{..........................}(6a),$$

and are now linear in h and c. We have now seven equations in which γ and r are known, h and c required. These equations, like the original equations, are weighted with weights B. In addition, however, they need another weight each, which we shall call Müller's weight. This we must next consider.

If we insert any pair of values h and c into the seven equations like (6a) these will also leave residuals v_2. If we were now to proceed to make

$$\Sigma Bv_2^2 = \text{a minimum},$$

this would be not only valueless but meaningless. For in the first place we cannot assume that the errors of the γ's are Gaussian, since we have already assumed this of the p's, and γ is such a function of p that the two assumptions are inconsistent. And in the second place we do not in any case want the most probable solution of these equations (6a) as they stand with their simple Bernoulli weights but of the equations (1). We must adhere therefore to the requirement expressed by equation (3), but we only have the v_2 residuals to work with. We require therefore a set of weights M such that

$$\Sigma Bv_1^2 = \Sigma BMv_2^2.$$

These weights Müller found to be

$$M = e^{-2\gamma^2}.$$

They are given in Müller's table of weights (to be found in various works on psychophysics). The products BM are Urban's weights, and we shall write in future

$$P = BM.$$

The conditions that

$$\Sigma Bv_1^2 = \Sigma Pv_2^2 = \text{a minimum} \qquad \text{.................}\ (3a)$$

are that

$$\frac{d}{dh}\Sigma Pv_2^2 = 0 \ \text{ for constant } c \qquad \text{.................}(4a),$$

and

$$\frac{d}{dc}\Sigma Pv_2^2 = 0 \ \text{ for constant } h \qquad \text{.................}(5a).$$

When these differentiations are carried out we get the Normal Equations[1]

$$[Pr^2]\,h - [Pr]\,c = [Pr\gamma] \quad \text{.....................(4}b\text{)},$$

$$-[Pr]\,h + [P]\,c = -[P\gamma] \quad \text{..................(5}b\text{)},$$

whence

$$c = \frac{[Pr]\,[Pr\gamma] - [P\gamma]\,[Pr^2]}{[P]\,[Pr^2] - [Pr]^2} \quad \text{..................(7)},$$

$$h = \frac{[P]\,[Pr\gamma] - [Pr]\,[P\gamma]}{[P]\,[Pr^2] - [Pr]^2} \quad \text{.....................(8)},$$

$$a = \frac{c}{h} = \frac{[Pr]\,[Pr\gamma] - [P\gamma]\,[Pr^2]}{[P]\,[Pr\gamma] - [Pr]\,[P\gamma]} \quad \text{...............(9)}.$$

In the example we are considering these formulae give the values

$$c = 13{\cdot}5676,$$

$$h = 0{\cdot}136113,$$

$$a = 99{\cdot}68 \text{ grams.}$$

4. *Urban's Calculation of its Probable Error.*

Professor Urban next proceeds to find the probable errors of these quantities thus. He calculates the probable errors w_c and w_h of c and h by the formula

$$w = {\cdot}6745\sqrt{\frac{\Sigma P v_2^2}{g\,(l-2)}} \quad \text{.....................(10)},$$

in which l is the number of equations, seven, and g is the 'weight' of c or h respectively and is found from the above expressions (7) and (8) according to rules which need not detain us here. Professor Urban thus obtains

$$w_c = 0{\cdot}91,$$

$$w_h = 0{\cdot}0092.$$

From these he obtains the probable error of c as follows:

$$a = ch^{-1},$$

$$\therefore\ w_a^2 = \left(\frac{da}{dc}\right)^2 w_c^2 + \left(\frac{da}{dh}\right)^2 w_h^2$$

$$= \frac{w_c^2}{h^2} + \frac{c^2 w_h^2}{h^4} \quad \text{.............................(11)},$$

whence on substituting the above values

$$w_a = 9{\cdot}477.$$

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[1] The square brackets are the sign of summation used by Gauss.

In these calculations there are two errors connected with equations (10) and (11) respectively.

1. Equation (10) is the formula given in text-books for ordinary sets of observation equations. But our equations have a unique peculiarity which makes this formula inapplicable, viz. the weights P are themselves functions of the observed quantities. For this reason the probable errors of c and h are incorrect.

2. Even were this not so, yet equation (11) is inapplicable because c and h are not independent. The result of this is that the probable error of a will be too large if c and h vary directly, too small if they vary inversely.

5. *Corrected Formulae for the Probable Error.*

Under these circumstances the only thing to be done is to go to the formulae (7), (8) and (9) and calculate the probable errors direct from them. We shall do this first for a. Equation (9) gives a ultimately as a function of the measured quantities p (for the P's and the γ's are themselves functions of the p's) and the quantities r. The latter are supposed to be subject to no error. We have therefore

$$w_a{}^2 = \sum_1^n \left(\frac{da}{dp_\kappa}\right)^2 w_{p_\kappa}{}^2,$$

where

$$w_p = \cdot 6745 \sqrt{\frac{pq}{450}}.$$

The differentiations are long and clumsy but otherwise present no difficulties. If we write N as an abbreviation for the numerator, and D for the denominator of the expression (9), we have

$$w_a = \frac{\cdot 6745}{D\sqrt{450}} \sqrt{\Sigma p_\kappa q_\kappa \left(\frac{dN}{dp_\kappa} - a\,\frac{dD}{dp_\kappa}\right)^2} \quad \ldots\ldots\ldots\ldots(12),$$

and

$$\sqrt{pq}\,\left(\frac{dN}{dp} - a\,\frac{dD}{dp}\right) = \mu\,(\gamma\alpha + \beta) + \xi\alpha,$$

wherein the following abbreviations are made:

$$\mu = \frac{P\,(p-q)}{\sqrt{pq}} - 2\gamma\sqrt{P\pi},$$

$$\alpha = r\,[Pr] + a\,[Pr] - [Pr^2] - ar\,[P],$$

$$\beta = r\,[Pr\gamma] - r^2\,[P\gamma] - a\,[Pr\gamma] + ar\,[P\gamma],$$

$$\xi = \tfrac{1}{2}\sqrt{P\pi}.$$

In these last five equations most of the letters should be read with suffix κ which is omitted for clearness.

The arithmetical work is tedious. The values on which it is based can be extracted from Tables 11, 40 and 42 in the article by Professor Urban already quoted. Calculating with seven figure logarithms without interpolation the writer obtains

$$w_a = 0{\cdot}151.$$

Similar formulae can be made for w_c from equation (7) and for w_h from equation (8). The three values are compared with Professor Urban's in this table:

w_a	0·151	instead of	9·477,
w_h	0·003	"	0·0092,
w_c	0·283	"	0·91.

6. *A Check Furnished by Subdividing the Data.*

These differences are so alarming in the case of w_a, the most important quantity, that it seems advisable to support our correction by a few considerations from first principles. When we say that the most probable value for a is

$$a = 99{\cdot}68 \pm \text{probable error},$$

we mean that the inconsistencies are such as to lead us to believe that if we repeated the whole experiment a large number of times under apparently the same conditions then probably half the values of a would lie within the above limits. Or we may look at the matter in another way. Suppose the data at each r to be divided into m chance samples, so that we could calculate m values of a. Then we may expect these to be so distributed that one-half of them lie outside $\sqrt{m}$ times the above limits. This in itself puts the larger value for the probable error, 9·477, quite out of court, for it is incredible that the result of such subdivision into say nine samples should give us nine values of a so scattered that four or five of them lie outside the limits

$$99{\cdot}68 \pm 3 \times 9{\cdot}477,$$

i.e. 71 grams to 128 grams,

when the whole range of comparison weights was only 84 to 108 grams and the subject hardly ever answered *heavier* at the lowest, or *lighter* at the highest of these. Moreover, some numbers supplied by Professor

Urban in another connexion enable an approximate value to be calculated on these lines, as a rough and ready check on our work. The experiments on Subject I from which the above numbers are taken were in fact performed in nine periods and Professor Urban has calculated a, h and c for each.

The values which he finds[1] are given in this table. It will be seen that the values of a are all within a range of three grams.

	h	c	a
	·1252	12·653	101·09
	·1126	11·083	98·47
	·1973	20·072	101·71
	·1109	10·976	98·94
	·1274	12·744	100·02
	·1382	13·846	100·20
	·1465	14·462	98·74
	·1245	12·211	98·10
	·1322	13·133	99·34
Means...	·1350	13·464	99·62

The means of these numbers do not differ much from the values obtained directly from the whole material: and their probable errors can be found in the ordinary way by finding and squaring the deviations. They are

$$w_a = 0{\cdot}276,$$
$$w_h = 0{\cdot}006,$$
$$w_c = 0{\cdot}613.$$

Now it cannot be expected that these values should be identical with those found from the whole material taken all together, for if there have occurred any changes due to practice or any other factor, the scatter of the above numbers will be greater than that due wholly to chance sampling. We may expect therefore that the probable errors just calculated are larger than the real probable errors, and this decides us in our confidence in the corrected formulae given in this article. Especially is this the case with the probable error of a. For this Professor Urban found 9·477, we find 0·151, and our last considerations show that it is undoubtedly of the order 0·276 and probably smaller than this. Professor Urban's chief error lay in assuming the independence of

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[1] F. M. Urban, "Einfluss der Übung bei Gewichtsversuchen," *Archiv f. d. ges. Psychol.* 1913, xxix. p. 276, Table 1, columns h_2, c_2 and S_2.

c and h, and an examination of the above table shows clearly that these quantities, here at any rate, have varied directly with each other.

7. *Conclusions.*

At the close of his article Professor Urban puts together in a small table[1] the values of the probable errors of the 'interval of uncertainty' for seven subjects calculated by the three processes. The averages of the columns of that table are given in the upper row of this small table. Only a long and careful re-calculation would enable the correct values to be given, but they are undoubtedly of the order of magnitude of the numbers in the second row. The $\Phi(\gamma)$ Process is therefore quite able to compete in accuracy with the others, indeed it may prove to be the most accurate. In forming a final judgment the probable errors of these probable errors must be borne in mind. The supposed inaccuracy of the $\Phi(\gamma)$ Process was attributed by Professor Urban to the fact that this function does not accurately express the relationship of r and p. As a set-off to this it must however be remembered that even a poor psychometric function has the advantage over direct methods of permitting the p's to be weighted with the Bernoulli weights B, and is therefore less liable to disturbance by variation in those p's which are most variable.

Lagrange interpolation formula	Limiting process		$\Phi(\gamma)$ hypothesis
	Calculated	Observed*	
0·28	3·68	4·36	8·63
0·28	0·3	0·3 +	0·3 −

* The word 'observed' is misleading. These values are also calculated but do not use up the data so completely. They are based on the ordinary Limiting Process, the 'calculated' values on Urban's formula.

Note. Extracts from the MS. of this paper were sent early in February to Professor Urban who was good enough to reply on February 27th pointing out difficulties in the way of his acceptance of the correction, the chief of which is that Professor Urban urges that c and h are independent. As this view rests on a misunderstanding the following notes are added to the galley-proof to make the relationship between c and h clear.

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[1] *Op. cit.* p. 415.

Imagine a solid model constructed with c, h and $e^{-\Sigma P v_2^2}$ as its three coordinates. To each point on the (c, h) plane belongs of course a value of c and a value of h, and if these are inserted in the seven equations like (6*a*), residuals v_2 are obtained from which $e^{-\Sigma P v_2^2}$ can be found. The model thus made is a hillock with its summit at the point where equation (3*a*) is satisfied, where the probability of c and h being the true solutions of (6*a*) is greatest. As current coordinates of the model c and h are of course independent, and equations (4*a*) and (5*a*) simply mean that we take vertical sections of the model to find its highest point. But the c and h which refer to this highest point, and which are the solutions of this last pair of equations, are not usually independent but correlated, and indeed the model itself is their correlation surface. If the whole experiment on Subject I be repeated under similar circumstances, then a new set of p's will be obtained from which a new model may be made, whose summit *may* come anywhere in the (c, h) plane: but the higher the first model is at any point, the greater the probability attached to that point as a site for the summit of the second model. If one hundred repetitions of the experiment were made the hundred summits would not be scattered anyhow over the (c, h) plane, but would in the case in question form an elongated cluster lying obliquely to the axes.

The contour lines of the model are found by equating $\Sigma P v_2^2$ to a constant. This gives

$$[Pr^2]\,h^2 + c^2 - [Pr]\,h\,.\,c - 2\,[P\gamma r]\,h + 2\,[P\gamma]\,c + [P\gamma^2] = k,$$

which represents a family of ellipses. There will be correlation between c and h as long as these contour-ellipses lie obliquely to the axes, and this will be the case as long as there remains a term in $h\,.\,c$. There will only be zero correlation therefore when $[Pr]$ is zero, whereas in our example it has the value 340·39.

These considerations suggest a crucial test of Professor Urban's formula. The comparison weights in this experiment range from 84 grams to 108 grams. There is no need however to measure them from zero in this way and we can if we choose measure them from any arbitrary zero. Indeed Professor Wirth has suggested such a change as a simplification and Professor Urban has published tables based on the plan. Now such a change will of course alter the absolute value of the threshold a, but if we keep the same unit it will not alter the scatter or the probable error. Accordingly, any formula for the probable error must be independent of a change of origin, and it will be found in truth that if in our formula (12) we write $r + x$ for r and $a + x$ for a, the x disappears. Not so in Professor Urban's formula. There is an obvious misprint in it on page 414 of his article, but it should read in full

$$w_a = \frac{\cdot 6745}{h} \sqrt{\frac{[Pr^2] + a^2 [P]}{[P][Pr^2] - [Pr]^2} \times \frac{\Sigma P v_2^2}{l - 2}},$$

and this is not independent of the position of the arbitrary zero as can be seen by the above test.

If the origin be chosen exactly at the point where $[Pr]$ equals zero, the correlation between c and h will vanish, and then the second and most important objection to Professor Urban's procedure will disappear, although the first by no means inconsiderable error will remain. The point 98 grams happens almost to fulfil this condition, and if we calculate with this as origin, we get on translating back to the old origin

$$a = 99{\cdot}68 \pm 0{\cdot}39.$$

So Professor Urban's formula can be made to give us any value from this upward by merely moving the origin, a procedure which ought to have no effect on the probable error, and has indeed no effect on the corrected formulae given in this paper. The minimum value obtainable by Professor Urban's formula is of course itself erroneous; it is the probable error which would result if on repeating the experiment we used the new values p but kept the old weights P.

(*Manuscript received* 30 *January* 1914.)

A NOTE ON PICTORIAL BALANCE.

By ROSSITER HOWARD.

There seems to be a persistent assumption that the right and left halves of a picture must balance, and doubtless even in apparently asymmetrical pictures there is a sort of 'hidden symmetry[1]' or 'occult balance[2].' But stated in terms of halves, the rule needs examination. Every rule of art is continually on its trial before the tribunal of our experience, and any exception would divest it of its dignity as a principle.

Now Velasquez's great portrait of *Innocent X* in the Doria Gallery appears to be one of such exceptions. True, it produces a feeling of balance not unlike what we experience before some triptych like Carlo Crivelli's *Madonna and Saints* in the Brera Gallery, from which we have a peculiar satisfaction clearly due to the easy rest of our attention upon the central figure, perfectly supported by the side panels; but in the Velasquez the point of rest, the face, is well to the left of the centre. The theorist has a ready explanation of this apparent exception; for he finds that there is a hand holding a white paper four times as far on the right of the centre, so that if it is only one-fourth as strong an attraction as the head it will still maintain the balance of the picture's halves,—on the principle of the Roman balance, in which the length of the lever-arm varies in inverse ratio to the weight. The figure is a good one as long as we remember that the pull of the attractions is toward themselves rather than toward the base, and that the idea of weight which we attribute to them is a secondary effect, probably one of association.

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[1] See Puffer for the experimental study of substitutional balance, *Psychol. Rev. Monograph Suppl.* IV., Harvard Studies I. There is a fuller interpretation of this study in *The Psychology of Beauty*, by the same author.

[2] Ross, *A Theory of Pure Design*. Both Ross and Puffer make the vertical axis of balance pass through the centre of the panel.

But in spite of this explanation of equivalence of forces on either side of a centrally placed fulcrum, do we not feel that in this portrait the head is the centre about which all the forces of attraction are ranged in support? For there it is, after all, that our attention finds rest, and not by any possibility on the central axis of the picture. Although this feeling of equilibrium gradually gives way before one attraction after another, inviting the play of the attention to a series of other points, after each such movement the balance is every time restored through the magnetic eyes of the pope. When a moment ago we considered the spots that attracted our attention, we concluded that the picture was bisymmetrically balanced; now that we consider the attention we give to the various parts, we find that the picture is balanced about a point to one side. We have implied that balance consists in equivalence of opposed attractions. It is clear that if the right and left halves of a picture are balanced, there cannot at the same time be an equivalence of attractions on either side of a point placed on one side. Our experience suggests that at first we placed the fulcrum of our lever in the wrong place; in the case of this portrait, it is, not in the centre of the field of the picture, but in the centre of our field of attention.

I think that if we examine our experience we shall find three distinct species of balance,—(1) the quality of vertical stability, (2) the balance of the field of the picture about an unmarked or slightly marked central vertical axis, and (3) the balance of the field of attention. The first species is present in every good design, but the second and third are possible together only at the moment during which their centres fall on the same axis. The element of time is important, for the attention is always moving or gathering a tendency to move, and (2) stimulates this movement, while (3) tends to suppress it.

At first it seems as if these species of balance were hardly co-ordinate, and yet they might all three respectively be resolved into compensated motor impulses,—(1) away from vertical stability, (2) right and left alternates, for they could be simultaneous only if the attention were centred between them, thus implying the third species, and (3) opposites occurring simultaneously. The attractions which cause these impulses may be utterly heterogeneous,—colours, actions, lines, represented movements, and facial expression, co-ordinated in our sense of balance.

The first species may be quickly disposed of. Supposing we draw a rectangle and within it two equal circles at equal distances from the base and from the centre. If we make one circle much smaller than the other our attention is held longer on the larger, and we then feel that side to be 'heavier'; we have lost our sense of equilibrium. However, we need only imagine the larger circle as a little fat man throwing a football to the ground, and the downward movement of the ball restores our feeling of uprightness. If the weight of the heavy circle is still too great, we can correct it by drawing a horizontal ground line, and we can add still more stability by doubling the lines of our rectangle as a strong frame for our picture. We have restored our vertical stability by a combination of unequivalent attractions, unequivalent representations of movement up and down, and an added suggestion of immovability in the frame,—heterogeneous enough elements of pure design and of representation working together in perfect accord.

To cite one example of the loss and gain of vertical stability among the old masters, let us mention Michael Angelo's *Last Judgment.* While the masses of the two sides are approximately equal, the movement on the left side is up while that on the right is down; so that we feel the wall to be spinning like a teetotum. But there is a copy of this fresco in the Naples Museum, made before the smoke of a thousand candles had blurred the composition, and this copy shows strong horizontal lines which lend stability.

Whether the disturbance of vertical balance in a picture arises because we associate pictures with material objects which we wish to restore to equilibrium when they tend to fall over, or because we feel ourselves tipping with them, it is certain that such balance is a negative advantage, required only because its absence is a positive discomfort.

Returning to our two equal circles in the rectangle, we find an interesting example of the second species of balance,—bilateral symmetry. Though the right and left sides are certainly equal, the attention does not rest at the centre between these two equally attractive spots; it jumps to and fro from one to the other, dissatisfied with either. We have movement rather than repose. By making one circle smaller we lose the equality, but by moving the larger circle nearer the centre we find at a certain point a kind of satisfaction that has been explained as resulting from an equivalence, rather than an equality, of right and left impulses. But notice that the attention does not even now feel at rest on either circle; it is simply held longer on one than on the other, and the to and fro movement takes on a more interesting rhythm. The feeling of easy swing towards the smaller circle may be balanced by the greater intensity of the feeling towards the larger; but we do not experience the sense of repose which we have when we remove the smaller circle altogether and balance our field of attention, now centred on the large circle, by the simultaneous right and left motor impulses of the longer free distance and the shorter distance intensified by the buffer effect of its outer end[1]. But the introduction of this third species of balance must not divert us from the second until we have found some examples in pictures.

The bilateral equivalence of sides, implying equal horizontal extent on either side of the axis of balance, seems to me much less common than is supposed, most painters avoiding it and taking the centre of interest so seriously that they balance the field of attention on the most important point. But Giorgione shows us some clear examples. Giorgione's characteristic type of composition is in the form of three interlaced triangles on a common base. An example is the *Apollo and Daphne* in the Seminario at Venice. The apex of the central triangle is at the centre of the panel, and the mass on the right is more important than that on the left and nearer the centre, other elements of the design tending to support this bilateral equivalence. But notice that our attention rests nowhere, but plays rhythmically across the panel, poising only an instant at the central point. It is only during that instant that the field of attention is in balance, and so strong is the suggested movement that we pass the suspension in easy rhythm, missing the third species of balance in the alternate motor impulses of the second. We find the same principles, with a slower movement of the attention, in Giorgione's *Adrastus and Hypsipyle* in the Giovanelli Palace at Venice.

This species of balance is so closely connected with rhythm and harmony, that Ross[2] caused some surprise among painters, proverbially loose in words and strong in sensations, by differentiating harmony, rhythm, and balance; and, indeed, Ross himself would make balance an element of harmony, as this species certainly is.

But we are still far from the experience we had before the Crivelli *Madonna* and the Velasquez portrait, where another, the third, species of balance, was a positive sense of continued suspension,—repose. This is the balance of our field of attention, and the point of rest may be anywhere within the panel as long as it does not bring the frame into our consciousness as an irksome limitation. In the Crivelli we find it placed in the centre, concentrically in regard to the centre of the field of the picture; in the Velasquez, at one side, eccentrically in regard to the centre of the field. In Andrea del Sarto's *Assumption of the Virgin* doubtless the head of Mary was intended to be the culminating point, but the upward look of the face in such close proximity to an unguarded frame carries our eye with a shock against the frame—indeed, as the picture hangs in the Pitti Gallery, our vision jumps the frame and rests on a white medallion on the cornice of the room. On the other hand, in Titian's *Assumption*, our eye, with no sense of limitation, is sent back from above to the head of Mary, to rest there longer, supported from below as well as on either side and from above, before it plays through the lower part and swings rhythmically back to the head, always under perfect control of the composition. Evidently this principle is important up and down as well as sidewise.

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[1] See Angier for experiments in the psychology of unequal division, *The Psychol. Rev. Monograph Suppl.* IV., Harvard Studies I.

[2] *Op. cit.*

We can simplify the problem, avoiding the intricacies of a painting, by the use of an imaginary diagram. Draw a rectangle, say three inches wide by two inches high, and within it draw a horizon line about three-fifths of the way up. Put a vertical line for a tree about three-fifths of the distance from left to right, making a mass of foliage rather more to the left than to the right, in the same proportion of three to two. We shall have a pretty respectable composition, an example of the 'golden section.' But now look at the diagram. The attention is surely caught by the tree. Let it rest there as long as it easily will. After a moment it plays over the horizon to the left, and on finding no rest it returns, only to repeat the performance in less time to the right, —and again the same rhythm, long and short, within the foliage mass, this time 'in diminution,' as a musician would say. Evidently, both in time and intensity, our experience of the right side of the diagram, as our attention rests on the tree, vastly outweighs our experience of the left side; yet the picture still seems balanced,—not, however, on its own centre, but on the centre of attention.

Or see Goya's paintings of a *Maja* in the Prado Museum; either will do, the nude or the draped. The figure of a girl reclines on a couch, her head and shoulders to the right on a pile of white cushions. In this case, though the right side far outweighs the left, its lever arm is just as long, and a little longer, than that of the left. Goya has controlled our attention by simple absorption in the object presented to it within an ample space. He has no need of bilateral symmetry. Though the surface of the figure is too large to afford a single point of repose, we have a loose balance of the attention.

And so in Degas's drawings, in which he makes us feel that his frame is like a window through which we have a perfect view of the action, or likelihood of action, within his panel.

Aesthetic repose we feel most deeply when the fields of the picture and of the attention are centred on the same axis, as in the symmetrical works of the Middle Ages. We are absorbed in the central figure; and so equal is the support on all sides that we feel no effort, but seem caught up off the ground. It is as though we stood in a great Gothic cathedral and looked far and high between the ever flowing lines of the architecture, lost in the radiance of the east windows. Certainly no mere perfection of balance can by itself perform such miracles of mood creation; nevertheless, it is the condition under which those miracles most frequently occur. For example, an intense and exalted idea, like that in Rembrandt's *Christ at Emmaus* of the Louvre, or of Fra Angelico's *Transfiguration* in San Marco, built into so powerful a balance, has an effect of blotting out the consciousness of all else but itself, the very consummation of art, as Hegel would have it, the reconciliation of the finite with the infinite.

But in more rhythmic compositions we are not wont to find such perfect rest, for our field of attention seldom coincides with that of the picture. Our attention rests on the centre of interest, to one side of the centre of the picture, but it increasingly feels the pull to the other side till it jumps clear over and then swings by easy stages back to its rest. To say that we look at a picture first as a whole is simply not to distinguish what we do, for our attention must be centred even if it be not concentrated. Supposing we let it play over the surface of the picture; as movement is always from lesser attractions to greater, it must reach the less after having been caught first—no matter how slightly—by the greater. As on a rose branch, our attention, first attracted by the rose, does not then painfully work its way down the branch against all the suggestion of movement, but follows somewhat the movement our hand would make in stroking the branch. This applies only to pictures that are near enough to effect a muscular movement of the eye-ball. A distant picture loses both in rhythm and balance; so we must assume that the picture fills a considerable angle of our field of attention.

It has been necessary to speak thus fully of movement because within the movement of a picture the eye finds momentary points of rest, auxiliary centres of balance, and these are arranged as effective supports for the principal balance. So in a van Dyck portrait we are likely to find the hands, two light spots, supporting the larger light of the face. (Cover one hand and the poise of our attention on the face becomes worse than precarious; cover them both, and the attention is not supported but is nailed to the face.) There is movement to and from the hands, which act as momentary centres, and van Dyck is careful to see that the movement is not straight from hand to face, but is given a melodic form by some curve of gold chain, touch of light on the sleeve, or other subordinate attraction. These subsidiary points of rest, then, partake slightly of the character of aesthetic repose, they affect the rhythm, and they repeat in diminution our experience of the principal repose, besides fulfilling their chief function as supports for the culminating balance of the attention on the head.

From the foregoing several facts seem clear:

First, we feel uncomfortable when we look on vertical instability, and when we find that quality in a design we say it is unbalanced; the inversion of that quality becomes balance, never a positive element in causing the enhancement of life in an aesthetic experience.

Secondly, we feel the pleasure of stimulated vitality when we look on a design in which the attractions of the right and left halves are equivalent but the principal point of interest on one side, and such an arrangement of attractions has been called balance, our second species.

In such a design our attention is never balanced; for when it is centred at the right it is pulled toward the right by the attractions of the right half of the design *minus* those that lie between it and the central axis, and it is pulled toward the left by the attractions of the left side *plus* those that lie between it and the central axis, making the tendency to movement greater than the tendency to repose. Such an experience at the right will shortly be repeated to the left in equivalence, but differing in time and intensity. The pleasure is interpreted as resulting from the fact that equivalence of right and left motor impulses is in harmony with our bilateral organism. Our experience is not distinctly one of repose but of harmony. May we carry the interpretation further, and say that this harmony is the pleasure of an unusually facile realisation of relations? When the design is on a circular field there is a certain position in which we feel it as most beautiful, and may not that position be determined by a balance of right and left motor impulses through which we most easily feel equivalence? For our pleasure, in cases where it is great enough to distinguish its character, is not simply one of agreeable experience, but of renewed vitality, like that which we have from a good drive in golf, explicable on the ground of unaccustomed capacity within ourselves.

Thirdly, any forced limitation of the field of attention disturbs that freedom which is necessary to every aesthetic experience; and to place the resultant of all the attractions at a point equally distant from the sides of the frame is a long step toward removing the limiting effect of the frame. But this principle could have been satisfied without any reference to balance, as Titian in his *Bacchus and Ariadne* uses a skilfully controlled movement to swing our attention just clear of the frame. Or it might have been satisfied by our third species of balance, that of our field of attention. Thus this principle itself does not seem to be one of balance, though its infringement makes us instinctively call for balance as a most convenient corrective.

Fourthly, repose of the attention is a powerful factor in creating that abstract condition of mind necessary to every aesthetic experience; and to balance our field of attention about a centre of interest creates such repose, perhaps through the stimulation of simultaneous opposite motor impulses which neutralise the suggestion of movement. Yet movement, itself an important element of beauty, is bound to follow in any case, and the designer controls it by making his balancing attractions not quite equivalent. By thus alternating movement with repose we gain an increased sense of both, the stimulation of the movement carried into the period of equilibrium. Increase of life is also given, perhaps through the experience of unwonted power of realisation, by clarity in the relations of all the ideas of the picture—lines, colours, lights, objects, or characters—and when to this is added the perfect rest of a balanced field of attention, the mind, before a supreme design, is conscious only of desireless, effortless, abounding life.

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AN IMPROVED COLOUR-WHEEL.

By N. CAREY.

(*From the Psychological Laboratory, University College, University of London.*)

During a series of experiments on some classes of school children, the writer had occasion to test their power of discriminating colour. The apparatus used was an ordinary colour-mixer on which two discs could be rotated. The colours to be discriminated were obtained by mixing two coloured paper discs not differing greatly in hue. White cardboard discs formed backgrounds, and the compound discs were kept together by means of the usual screw. One of the discs was kept constant, while the other was varied. The children were required to look at the rotating discs and then to write down which of the two original colours the variable contained most of, as compared with the constant; for example, suppose the colours to be discriminated were obtained by mixing yellow and orange in varying degrees, the children would write down whether the variable were more yellow or more orange than the constant.

The results obtained by this method were extremely unreliable. This was attributed to three causes. First, a rather large chance error was involved owing to the judgments being limited to one of two possibilities. Secondly, a comparatively long interval of time between the exhibitions of successive members of a series was taken in changing the variable; this made even a short series of six judgments unduly long, so that some children invariably became inattentive, especially in the later series. Thirdly, the judgments required the naming of the colours, and this afforded difficulty in the cases of those children who were uncertain of the names applied to colours.

In order to decrease the first source of error, and to remove the third, the number of discs involved in each judgment was increased from two to five. In order to decrease the second, it was necessary to devise some means of dispensing with the screws, the manipulation and slipping of which were the chief causes of the lengthy time interval. The accompanying figure shows the apparatus devised to eliminate these errors. Four spindles *A*, *B*, *C* and *D* were arranged on one level, while a fifth *E* was mounted above them. Instead of the usual screw which is used to keep the disc in place, a small peg *p* (enlarged in figure B) was made to protrude from the back of the spindle, running parallel to,

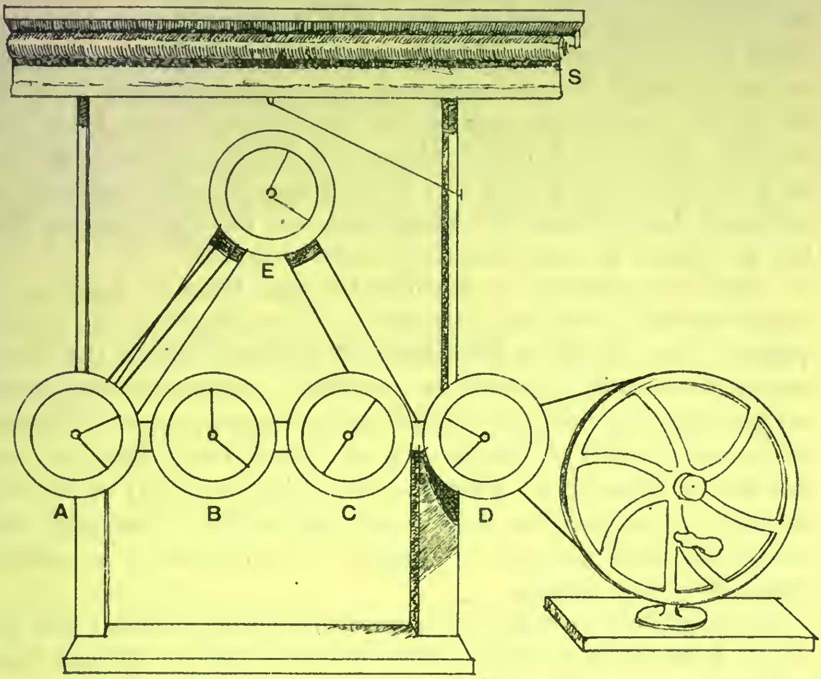

FIG. A.

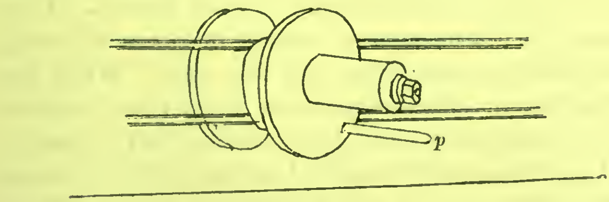

FIG. B.

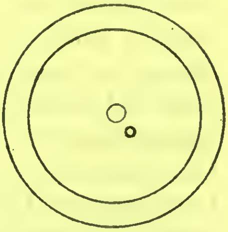

FIG. C.

and about half an inch from, the spindle itself. In each disc two holes had now to be punched, a larger one to take the spindle, and a smaller one to take the peg (Fig. C). The small hole was obtained at a correct distance from the larger one by means of a small brass disc, having a protrusion at the centre which fitted into the larger hole, and a small hole at the required distance away from the central protrusion, into which the small punch could be inserted. The spindles and pegs were of such a length that six cardboard discs at a time could be placed on them. On rotation, contrary to expectation, it was found that the discs showed no tendency whatever to fly off. As there was now no screw, the coloured discs had to be gummed to the centre of the cardboard discs forming the background, and therefore separate discs had to be made for every variation of colour required.

With the apparatus thus modified, the time taken in changing the colours was much reduced, as the discs could be slipped on and off very quickly. The increase in the number of the discs reduced the chance error, and therefore increased the reliability. Such an increase would not have been workable with the ordinary screw arrangement. Although this method entails the preparation of at least thirty discs, yet even this has its advantages. First, the two colours are likely to be more accurately proportioned, as they are arranged at leisure. Secondly, they do not get soiled since, once the discs are made, there is no need to handle the colours again.

The experiment was carried out in the following manner: The top disc *E* served as a constant, while the four discs underneath were variables, one of which was made to correspond with *E*. In order that the discs should be seen only during rotation, the apparatus was provided with a spring blind (*S*, Fig. A), which could be made to cover or uncover the discs by jerking a cord attached to the bottom. Thus during an experiment, the experimenter rotated the discs with one hand and pulled the blind up and down with the other. When the apparatus was in rotation, the blind was drawn up and the children were required to write down which of the four variables they judged to be the same as *E*. As six discs could be placed on each spindle, a series requiring twelve judgments could be given by merely pulling off the uppermost discs, and thus alternately changing the constant and the variables. A longer series of twenty-four could also be obtained by using twelve constants. This would necessitate the slipping on of a second set of six discs after the first six had been used; but as this process does not take very much longer than the removal, the loss in time is not serious.

It would be possible to obtain a series of even greater length by judiciously arranging the changes between the constant and the variables. By decreasing the differences between the variables, the series could be made of increasing difficulty.

The improvement from the point of view of results is obvious from the fact that by the first method a series of 70 judgments gave a reliability coefficient of ·38, while by the second method, a series of 36 judgments gave a reliability coefficient of ·80.

Many other uses of these innovations, especially of the pin in place of the ordinary screw, will readily suggest themselves, as for example, for class demonstration where the time lost in screwing and unscrewing is very inconvenient.

(*Manuscript received* 23 *March* 1914.)

A STUDY OF THE INDIVIDUAL DIFFERENCES IN ATTITUDE TOWARDS TONES.

BY C. S. MYERS WITH CONTRIBUTIONS BY C. W. VALENTINE[1].

I. *Introductory.*
II. *Experimental methods.*
III. *The varieties of aspects* [by C. S. MYERS and C. W. VALENTINE].
IV. *The relevant attributes of the sounds.*
V. *More detailed consideration of the aspects.*
VI. *Comparison of the attitudes towards colours and sounds.*
VII. *Comparison of the attitudes towards single tones and single bichords.*
VIII. *Comparison of the attitudes towards single bichords and pairs of bichords.*
IX. *Relation between aspect and aesthetic appreciation.*
X. *Relation between aspect and aesthetic appreciation* (*continued*) [by C. W. VALENTINE].
XI. *Appendices*
  a. *Other determinants of aesthetic appreciation.*
  b. *Indications of sexual and racial differences* [by C. W. VALENTINE].
  c. *Changes in experience while listening.*
  d. *The nature of the interval.*
XII. *Conclusions.*

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[1] I wish to express my gratitude to Dr Valentine—who has been working on the same subject simultaneously, but independently of me—for permitting me to incorporate some of his experimental results with mine in this paper. Originally these results formed part of a section of the paper which he submitted to me in September, 1912, for publication in This *Journal* (VI. 190—216). Dr Valentine, however, kindly consented to omit this section at the time, so that we might embody our results later in a single paper. This seemed desirable, since (i) in the previous March I had already read a paper before the Berlin Congress of the *Gesellschaft für experimentelle Psychologie*, in which I gave a *résumé* of my results, and (ii) in the section of his paper above referred to, Dr Valentine had quoted from the abstract of my communication which I had sent him. C. S. M.

I. INTRODUCTORY.

THE experimental results described in this paper are to be regarded as preliminary to an inquiry into the individual mental differences underlying man's attitude towards music. On general grounds it seems desirable to investigate the effects of such simple material as single (or paired) tones before making experimental use of the far more complex arrangement of tones employed in music. No *a priori* assumption is thereby implied that single tones (or pairs of tones) must make the same impression on a subject as would, say, a song or a symphony; indeed the difference between isolated tones and a piece of music must be at least as great as that between isolated words and a poem. Nevertheless interesting differences in the attitude of different subjects to single tones (or pairs of tones) may reasonably be anticipated and these may be expected to throw light on the nature of the well-recognized (but imperfectly studied) individual differences in attitude towards music.

The word 'attitude' is here used in its ordinary comprehensive sense, so as to include the whole of the resulting changes in consciousness,—that is to say, not merely the more immediate perceptual and affective results of the stimuli, but also the images and ideas, and the tendencies to action, which they more or less indirectly evoke.

II. EXPERIMENTAL METHODS.

The sounds used in these experiments[1] were produced from ten tuning-forks the tones of which ranged from 400 to 1300 vibrations per second, the pitch of each fork differing from that of its neighbour by 100 vibrations. The stem of each fork was horizontally placed in a support so that the two prongs lay vertically one above the other. Below the lower of the two prongs of each fork was fixed a stout but flexible steel band carrying on its upper surface a rubber knob. This steel band could be depressed by a simple mechanical contrivance and suddenly released. On the rebound of the band, the rubber knob on its upper surface struck the fork and threw it into vibration. The loudness of the tone depended on the distance through which the steel band was depressed. As this could be kept constant, the uniformity of the loudness of the tones was insured throughout the series of experiments.

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[1] Dr Valentine's procedure will be found described in his paper in This *Journal* (VI. 191, 200).

Such an arrangement of the forks was surprisingly successful in disguising the nature of the sound-producing instruments. Most of the subjects went through the experiments without knowing that the tones they heard were being produced by tuning-forks; several of the others only realised this towards the close of their sittings. The forks were always placed behind the subject. Each subject was investigated separately, some on two or more occasions. Each sitting lasted about three-quarters of an hour. In all, twenty-nine subjects were investigated, twenty-two men and seven women; they were drawn from University students, graduates and their wives. The subjects possessed all degrees of musical ability, careful note being taken of their tastes and proficiency.

The defects of this apparatus may be thus summarised: (1) such pure (overtoneless) tones as those emitted by tuning-forks, were of dull timbre and hence relatively uninteresting as compared with the richer sounds of musical instruments. (2) The production of the tones was unavoidably accompanied by the knocking noise of the steel band striking the fork. (3) The forks of different pitch 'rang off' at different rates, in consequence of which the experimenter had to silence them by damping them with his hands at the end of a constant interval (about three seconds) after they had been thrown into vibration. (4) There was inevitably a scientific atmosphere about the experimental procedure which prevented the subjects from taking up the desired aesthetic attitude so readily as when listening to a musical composition. But these drawbacks,—the dull quality of the tones, the noise of the knocks, the silencing of the forks, and the experimental atmosphere,—did not prove as serious as might *a priori* be expected. As a rule the subjects soon habituated themselves to them. It was only rarely that complaints were made of the want of interest in the timbre, the disturbing effects of the knocks, the disappointment at the too abrupt or premature termination of the sound, or the difficulties inherent in listening musically to such relatively meaningless material in the surroundings of the laboratory. Indeed these complaints, dependent as they were on individual mental differences, proved to be in themselves of psychological interest.

With rare exceptions (which will be noted later), the sittings began with the sounding of single isolated tones. Each time after a tone had sounded, the subject was encouraged to give as full an account as possible of his general attitude towards it,—what came into his mind when he heard it, what he thought of it, whether it was pleasing or displeasing and, if so, why, etc. Leading questions were carefully avoided; all that was, as a rule, found necessary, after the subject had grasped what was required of him, was for the experimenter to repeat encouragingly, "Yes, anything else?" as he recorded the introspections which were dictated to him.

After about eight single tones had been sounded, the subject was plied with pairs of simultaneous tones, or bichords[1]. Each bichord was sounded by itself and the subject received the same instructions as had been given to him for the single tones—namely to give a full account of his attitude. After about sixteen such bichords had been given *singly*, bichords were given *in* (about the same number of) *pairs*, the subject being asked again to describe his experiences as fully as possible, especially as regards his preference for one or other member of a given pair. In some of the earlier experiments, similar preference judgments were invited for pairs of successive single tones.

The single bichords employed included those occurring in music and also those not thus employed. The following is a specimen list of the bichords submitted to a subject:

$\frac{400}{700}$, $\frac{500}{600}$, $\frac{1100}{1200}$, $\frac{500}{800}$, $\frac{600}{900}$, $\frac{600}{1000}$, $\frac{300}{400}$, $\frac{600}{1200}$, $\frac{300}{500}$, $\frac{800}{1000}$, $\frac{1000}{1200}$, $\frac{400}{500}$, $\frac{1200}{1300}$,
$\frac{400}{600}$, $\frac{600}{800}$, $\frac{800}{1200}$.

As an example of the pairs of bichords presented, the following will serve:

$\frac{600}{1000}\ \frac{800}{1200}$, $\frac{500}{800}\ \frac{500}{600}$, $\frac{600}{800}\ \frac{400}{600}$, $\frac{1200}{1300}\ \frac{1100}{1200}$, $\frac{600}{900}\ \frac{600}{800}$, $\frac{400}{700}\ \frac{800}{1000}$,
$\frac{500}{800}\ \frac{600}{800}$, $\frac{1000}{1200}\ \frac{600}{1200}$, $\frac{500}{800}\ \frac{300}{500}$, $\frac{400}{500}\ \frac{500}{600}$, $\frac{600}{1000}\ \frac{500}{800}$, $\frac{500}{600}\ \frac{400}{700}$,
$\frac{500}{800}\ \frac{600}{1200}$, $\frac{600}{800}\ \frac{600}{900}$, $\frac{500}{800}\ \frac{300}{400}$, $\frac{600}{1200}\ \frac{500}{600}$, $\frac{400}{600}\ \frac{600}{800}$.

III. THE VARIETIES OF ASPECTS.

Before attempting to discuss in detail the individual differences in attitude recorded by the subjects of these experiments, it will be convenient to summarise the main aspects from which a given tone (or bichord) may be regarded. They are four in number,—the 'intra-subjective,' the 'objective,' the 'character' and the 'associative' aspects.

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[1] 'Bichord' is used throughout this paper as a convenient word for denoting any two simultaneously sounding tones.

Of these terms the three last have been already employed by Bullough in his similar experiments with colours[1], and are here used in a sense similar to that in which they were employed by him. That is to say, the objective aspect of a sound consists in the relation of the sound to the subject's standard of purity, pitch, etc., which a satisfactory sound should attain; the character aspect arises from the subject's tendency to personify tones, *i.e.* to endow them with human attributes and to regard them as distinct living entities; the associative aspect yields the various ideas (with or without concrete or verbal imagery) which a given sound may suggest. But in this paper the connotation of Bullough's objective aspect has been extended to include *any* attitude which induces a *passive* regard of the sound as having meaning or use as an independent object. Moreover, Bullough's remaining aspect, the physiological aspect, has been here extended to include not merely the sensory effects and the changes in feeling (emotion, mood or feeling attitude[2]) but also the experiences of *self-activity* which the sounds may produce in the subject; in consequence, the word 'intra-subjective' will be substituted for this aspect and Bullough's term 'physiological' will be limited to the first three of its five sub-aspects.

It will be convenient, then, to sub-divide these aspects according to the following scheme; the examples, illustrating these sub-divisions, have been derived from the introspective data of the subjects of the experiments.

I. Among the 'intra-subjective' changes must be recognized:

(*a*) Changes in the cognitive aspect of the subject's consciousness.

- (i) Leading him to term the sound, *e.g.* strident, piercing, hissing, hard, firm, velvety, soft, solid, rigid, round, broad, pointed, thin, luscious, mellow, insipid, acid, etc. (*physiological sub-aspect a*).
- (ii) Exciting in him, definite tactual, motor, or organic sensations, *e.g.* tactual sensations in the tympanic membrane, stinging in the arm, tingling down the spine, thrills, shivery feelings, going through the head (*physiological sub-aspect b*).

(*b*) Changes in the emotional state, mood, or feeling attitude of the subject, as instanced in feelings of desolation, dissatisfaction, depression, annoyance, aggravation, irritation, hurry, restlessness, laziness, repose, delightful surprise, being carried away to a region of mysticism (*physiological sub-aspect c*).

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[1] This *Journal*, 1908, II. 406–463.

[2] Equivalent to the German *Bewusstseinslage*.

(*c*) Changes of a conative nature in the subject's consciousness as seen in:

(i) Motor impulses,—*e.g.* impulse to turn the head above and to the right, to bend low, to stop it, tendency to keep the head moving (*conative sub-aspect a*).

(ii) Endeavour to determine the meaning or use of the sound, *e.g.* trying to connect it with piano sound, or with preceding sound, wondering what it suggests, inability to fix it in a scale, trying to fix its pitch or its interval, wondering if such a sound could come into a tune (*conative sub-aspect b*).

II. Under the 'objective' aspect must be recognized:

(*a*) Appreciation of the sound as having meaning or use, the sound being regarded

(i) As if it were language,—*e.g.* an admonition, a question, an affirmation;

(ii) As if it resembled the behaviour of some other (non-musical) sound,—*e.g.* a throb, an echo;

(iii) As if it were a musical note,—*e.g.* about F♯, a note in Church music, a leading note, a major third.

(*b*) Consideration of the sound in relation to the subject's standard of purity, pitch, etc., which a satisfactory tone should attain,—*e.g.* a small sound that ought to be big, too loud for that particular quality of sound, below what I should have liked, not what I should have expected, this interval not suited to the *Glockenspiel*, died away too quickly, horribly short, a nice tone, a good harmony, needs resolution, I pass it, rich, poor, beating, reedy, metallic.

III. Under the 'character' aspect are included such 'anthropomorphic' replies as pretty, gentle, solemn, grave, sedate, dignified, mysterious, stupid, silly, sad, horrid, grotesque, bony, bare, angular, clumsy, *piquant*, fidgetty.

IV. Under the 'associative' aspect are included:

(*a*) Suggestions of an instrument, *e.g.* gong, finger bowl, cathedral organ, fog siren, keys of pianoforte, alarm bell, musical box;

(*b*) Suggestions of music, *e.g.* the opening chord of the Dead March, a certain passage in 'Tannhäuser,' a note in one of Brahms's *Intermezzi*, it might be the end of a chorus, the Valse in 'Faust';

(*c*) Suggestions of surroundings in which the instrument or music is sounding, *e.g.* a finger bowl of definite shape upon a table, a ventilating fan in a railway carriage, a fog siren heard from the promenade at F—, ascending mountain in a train with distant whistle of the engine, someone dressed in black clothes and with dark hair bending over a violoncello, myself holding a table fork and striking it on the table, atmosphere of boy with guitar in Spain, someone clumsily dropping his hands on the keys of an organ within the dimness of a church, a man playing the organ in C— college chapel.

(*d*) Symbolic suggestions, *e.g.* the head and beak of a parrot looking its silliest, a narrow stream of grey light in movement, a halo, looking through a misty grey limitless veil, feeling something opening and something big emerging of which I could not get hold, a cylinder, a prism with its edge upwards, a bilaterally asymmetric figure, a plane, a circle crossed by lines in all directions, pushing my finger into an unresisting solid.

IV. The Relevant Attributes of the Sounds.

The attributes of the tonal stimuli, influencing the attitude of the subjects in these experiments, may be considered under five heads,—duration, loudness, pitch, timbre, and tone-combination.

Duration. As has been said (p. 70), the duration of the tones was objectively constant. Notwithstanding this, duration played a part in influencing certain subjects. For, owing to varying attention and interest, one tone appeared to last longer or shorter than another, so that occasionally a subject commented on the tone being "horribly short" or remarked that "you stopped it too soon," etc. (cf. p. 76).

Loudness. Here again, although every effort was made that the tones should sound with equal objective intensity (cf. p. 69), we have an attribute which cannot be left out of account. For it was impossible to prevent the tones of different pitch from 'ringing off' at different rates. Hence the attention of the subject was occasionally drawn either to the rate of decreasing loudness of a given tone, or to the loudness of the tone at the moment it was silenced after a constant interval. Moreover, loudness, it must be remembered, is not merely dependent on the physical strength of the stimulus; it also involves at least two, individually variable, psychical factors. The first of these is connected with the varying 'aggressiveness' of tones according to their pitch; in consequence of which high tones appear louder than low tones, even when the stimuli are of equal objective strength. The second subjective factor consists in apparent variations in loudness of a tone which is objectively of constant strength; a tone may often *appear* to gain or to lose in loudness after it has first been heard (cf. p. 108).

Pitch. Tones of different pitch vary in what has been termed tone-character[1]. Such variations are generally described by the subjects in terms of cutaneous and motor experience, very high tones appearing 'fine,' 'pointed' and 'light,' very low tones appearing 'massive,' 'round' and 'heavy.' Tone-character may be also expressed in visual terms (high tones being described as 'bright' or 'glaring,' low tones as 'dark' or 'sombre') and perhaps (but far less frequently) in other sensory language.

The origin of such experiences is still a matter of dispute, but one 'explanation' can at once be dismissed. It has been repeatedly suggested that they are the result of association,—that high tones, owing to their being produced from small brass instruments, appear pointed and bright, whereas low tones, being produced from ponderous dark-coloured instruments, come to be regarded as massive and sombre. We shall later discuss the subject more fully in connexion with auditory synaesthesia (p. 81).

Not infrequently subjective changes of pitch may be experienced in listening to a given sound (cf. p. 107). The tone appears to rise or to fall in pitch, showing a behaviour similar to what has been just described under the heading 'loudness.'

Timbre. Even tuning-forks fail to produce a tone entirely free from overtones. Moreover, since in daily experience it is impossible to alter the pitch (or loudness) of a tone without changing its timbre (or *vice versâ*), it is not surprising that the effects of changes in timbre and in pitch are difficult to separate. The most characteristic expressions alluding to timbre are 'richness,' 'warmth,' 'brightness,' 'softness,' and their opposites.

Tone-combination. Between timbre and tone-combination there is probably a gradual transition, depending on the relative strength and pitch of the tones present, and perhaps on the analytical powers of the subject. The absence of accompanying tones, whether they be loud enough to be regarded as fundamental tones or weak enough merely to give the experience of timbre, might *a priori* be considered to be the basis of judgments of 'purity.' But it is doubtful whether even a highly musical person would *necessarily* call a hypothetically pure tone, which has been absolutely freed from accompanying tones, absolutely pure. Purity in sound, as in colour, is a relative term and is dependent on various standards; the timbre of the human voice, the timbre expected from the instrument producing the sound, the particular pitch of the note, are all factors, *inter alia*, influencing the subject's standard of purity at any moment.

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[1] Cf. my *Text-book of Experimental Psychology*, 2nd ed. Cambridge, 1911, I. 32 ff.

It is curious how often in these experiments the less musical subjects utterly fail to notice that two tones are present until their attention is directed to it. They consequently treat a bichord just as if it were a single tone, describing it in terms of richness, fulness, etc., and their opposites, or alluding to its sensory or affective effects on themselves. Even such attributes as clearness or blurredness may be employed by subjects who are nevertheless unaware that more than one tone is present. The recognition of the presence of more than one tone, at once yields answers in terms of 'fusion,' 'blending,' 'harmony,' 'discord' and the like.

V. MORE DETAILED CONSIDERATION OF THE ASPECTS.

We pass now to further illustrations of the various aspects already enumerated on pp. 72–74, and to a consideration of the inter-relation of these aspects.

1. *The Intra-subjective Aspect.*

The Physiological sub-aspects:

The following replies illustrate sub-aspects *b* and *c*. Sub-aspect *a* will receive later treatment in connexion with synaesthesia (p. 81).

H. "I felt a touch on the tympanum." "I felt a stinging up the right arm as if the first finger touched a copper spring that rebounded." "I felt warm in the ear." "I had a lazy feeling." "It was aggravating, irritating."

G. $\frac{400}{700}$ $\frac{1000}{1200}$. "Gave me a tingling at the end of the right thumb, like what occurs when attention is directed to a part of the body." [This continued for some minutes and finally prevented further experiments. After a short walk outside the laboratory the experiments were resumed, but almost immediately the tingling returned.]

Aa. $\frac{400}{700}$. "I just listened to it. I liked it and was sorry when you stopped it."

$\frac{600}{800}$. "I liked it. I had a distinct feeling of annoyance when you stopped it."

$\frac{800}{900}$. "It was too beautiful. A sense of being uplifted, as if I were listening to it with the very tips of my ears."

The physiological sub-aspects are thus essentially passive aspects. The subject surrenders himself to the effects of the sounds. Contrast the self-activity inherent in the conative sub-aspects.

The Conative sub-aspects:

Psychologically the two sub-aspects *a* and *b* are nearly akin, both involving self-activity, the former of a lower impulsive, the latter of a higher intellectual type. From the affective and aesthetic standpoints, however, the former is closely related to the physiological sub-aspects, the latter to the objective aspect. Feeling an impulse to turn the head easily passes over into definite motor sensations, whereas such a professional attitude as trying to determine the pitch, or wondering what is the meaning or use of a sound, is clearly apt to result in the objective aspect from which the sound is regarded as an independent external object having definite meaning or use.

The following replies will serve to illustrate further the conative sub-aspect *b*:

J. 1200. "I wondered what was the difference between this and the last."

300. "Pleasant, but I cannot say why I think so. I tried to connect it with piano sounds but could not."

900. "It puzzled me, and made me wonder if it was a sound that could come into a tune."

$\frac{600}{800}$. "I can't join it on to piano sounds; I try to imagine it in its place in a piece played by a pianola."

$\frac{600}{1000}$ $\frac{800}{1200}$. "I began to think what relation they are to one another, whether one is an octave of the other or how far they are apart."

C. $\frac{500}{700}$. "It needed resolution, and became pleasant when resolved. I kept the top note constant and raised the lower."

$\frac{700}{800}$. "Unpleasant. I couldn't do anything with it. I wondered if it was a semitone."

R. 600. "I tried to see if the sound meant anything. It didn't."

V. $\frac{500}{700}$. "Pleasant. I wonder what it suggests, but cannot say."

$\frac{1100}{1200}$. "Ugly. I don't see what is coming next."

$\frac{700}{800}$. "Unpleasant. Ugly in itself. A discord. I wondered how it would resolve, what would happen to it."

N. $\frac{500}{800}$. "I am, as usual, thinking of the relation. A minor sixth. I saw *f* and *d*♭ but I am inclined to correct it to *e* and *c*. I cannot feel satisfied until I have determined the relations (this comes first) and their pitch."

R. $\frac{600}{800}$. "I cannot help trying to see what they are, because I am always analysing sounds."

V. $\frac{600}{800}$. "I was busy making it out."

2. *The Objective Aspect.*

Tonal, like visual, stimuli may be considered not only from the standpoint of their subjective effect on the recipient, but also as external entities. When the sound is regarded as an independent inanimate *object* we term the recipient's attitude 'objective'; when, on the other hand, the sound is regarded as an independent living *subject*, we say that the recipient adopts the 'character' attitude. The objective aspect comes to be naturally adopted by unmusical subjects who fail to experience any sensory, affective, or conative changes or who fail to regard the sound as endowed with such activities. *Faute de mieux*, the musical, as well as the unmusical, subject regards a sound merely as lifeless external object. In general an aesthetic experience cannot be obtained from this aspect. But under certain conditions the objective aspect may quite well serve as the basis of an aesthetic experience. Subject *S*, for example, who is in a very low degree musical, states that she always tends to "read poetry into the beautiful," and accordingly translates into verbal effects the sounds used in these experiments:

1200. "A sad haunting sort of echo. I like it."
300. "Like an admonition,—quite outside myself."
1000. "Very plaintive, like a question."
$\frac{300}{700}$. "A worrying question. Perplexing."
$\frac{500}{600}$. "Rather the opposite of a question: It seems to affirm something."

To this subject the tones appear as symbols, and hence it is not surprising that they may have an aesthetic value and an activity of their own. But even when the tone is considered as a *non*-symbolic concrete object, it may have in itself an activity or its relation with neighbouring tones may have an activity that evokes aesthetic appreciation. And even when it is not endowed with such activity, the sound, although treated from the objective aspect, may yet possess an aesthetic value. The frequently intellectual outlook of a musician on the material presented to his ear, is a clear proof of such an occurrence. The experience of beauty is hence not determined solely by the character (and intra-subjective) aspects; it may also be based on the purely objective aspect.

That the relation between tones may be considered as analogous to objective activity will appear from various illustrations given in the course of this paper. In Subject *I*, "*the music* forms (it is not *I* that form) patterns while I hear it, though I feel myself to some extent concerned in their movement[1]. The pattern is in three dimensions, up and down (corresponding to variations in pitch) and in a third plane (corresponding to volume)." So in passing from $\frac{500}{600}$ to $\frac{500}{800}$, when the two bichords immediately follow one another, there is a "movement of ascent as with all melodies. The sounds move, but yet no spatial quality is involved. There is no pattern here; the music itself is the pattern; nor is there any image."

In dealing with the objective aspect, in connexion with these experiments, it is interesting to note that in all four subjects, who complained of the knocking sound inseparable from striking the fork, this aspect predominated.

F. 500. "The 'attack' is obliterated by the knock. The knock bothers me."

G. 300. "I am disturbed by the hitting noise."

$\frac{300}{400}$ $\frac{500}{600}$. "I am much disturbed by the mechanism of producing the sound."

V. $\frac{700}{800}$ $\frac{1000}{1200}$. "I object to the click of the striking. I think it more noticeable in the dissonant intervals. Minor third preferred."

X. 600. "The knock is very disturbing."

500. "The sound is at first obscured by the knock. The knocks detract from the naturalness of these sounds and make them rather laboured."

3. *The Character Aspect.*

It might be thought, and indeed it has been suggested[2], that the character aspect is derived from the intra-subjective aspect—that a sound comes to be characterized as 'gentle,' 'clumsy,' 'fidgetty,' 'grave,' etc., because it evokes or might evoke *in the subject* impressions of this kind, making *him* feel gentle, clumsy, fidgetty, grave, etc., and causing him to project these experiences as characters of the object.

This, however, is surely open to question. When a person, in whom the character aspect is well developed, regards a colour as 'dishonest' or a sound as 'stupid,' he does not in the least feel dishonest or stupid himself. Such characters as 'jolly' or 'high-spirited' may be ascribed to a sound or colour by the subject even under conditions when he himself feels sad or depressed; and in consequence of such antipathy he will shun that particular sound or refuse to wear that particular colour. Moreover, it would be ridiculous to suppose that sounds bearing such characters as 'bare,' 'bony,' 'stupid,' 'humble,' 'trivial,' 'unfortunate,' or 'futile,' could evoke, or must necessarily ever have evoked, corresponding experiences in the subject himself.

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[1] This doubt as to how far the music and how far the self is concerned in the activity of the 'patterns' exemplifies the close relation (already noted on page 77) between the objective aspect and the conative sub-aspect *b*.

[2] Cf. Bullough, *op. cit.* 446 ff. He, however, distinguishes *two* processes in the derivation—(*a*) the 'exteriorisation' of the physiological aspect and its 'objectivation' as a physical attribute, and (*b*) the translation of the physical attribute, by 'empathy,' into a psychical feature of the object.

The independent origin of the character aspect is supported by the following extracts from the reports of Subject *Z*, in whom this aspect was well-marked. They show that the intra-subjective aspect, so far from dictating the character aspect, may arise from it.

300. "I felt a sound which was very strong and masculine, with a strong individuality about it. Just as I feel some robust sensation after exercise, so I felt towards this sound. *The masculinity of the sound suggested to me this condition of my person.* There was nothing displeasing in the sound."

700. "Quite pleasant since it was very light-hearted, without any really deep emotion. *When I felt the sound light-hearted, I became light-hearted almost simultaneously.*"

$\frac{600}{800}$. "Rather unpleasant, because it was a sound that had no emotional feeling in itself at all."

$\frac{1000}{1200}$ $\frac{500}{800}$. "The first was miserable, like insects dying and *hence gave me a feeling of rigidity.*"

Another suggestion has been often made that the character aspect is derived from association, a sound coming to be described as 'grave,' because of the suggestions to which it gives rise.

Bullough has already sufficiently urged various objections to this explanation[1]. Moreover, in two subjects, *N* and *X*, of the present experiments, precisely the converse process is demonstrable, the character inducing an association—'stupid' for example, suggesting 'a stupid parrot,' 'materialistic' suggesting the idea of 'a successful tradesman.'

Although, I believe, the *origin* of the character aspect is to be sought neither in the conscious projection (or 'distancing') of actual or potential subjective feelings nor in the conscious transference of associated experiences, there can be no doubt that the character aspect may occasionally be *determined* by such intra-subjective and associative influences. It has happened not infrequently in these experiments, especially in the case of subjects in whom the character aspect is weak or seldom manifested, that a doubt arises in their minds whether such a quality as sadness is experienced merely in themselves, or whether it really (or also) belongs to the sound. In the process of 'distancing' to which Bullough has drawn attention[2], the required attitude of impersonal detachment towards one's feelings (so essential for aesthetic experience), is attainable either by regarding them almost from the standpoint of the on-looker, while still recognizing them as subjective, or by consciously projecting them into, and more or less identifying them with the sound itself. Nevertheless, in those subjects in whom the character aspect is *well developed,* there is no conscious process of projection of this kind. The sound is treated by them as having personality from the outset, and, simultaneously with its perception, is endowed by them with one or more anthropomorphic characters. They do not actually feel these characters in themselves before they ascribe them to the sound, nor do they project these characters into the sound by a process of 'distancing' before they actually experience them in themselves, any more than they would necessarily feel them actually or potentially in themselves before ascribing them to a *person* whom they would characterize as honest, light-hearted, and so on. The characterization of sounds among ourselves has the same basis as animism among primitive peoples; both arise from the innate tendency to treat all objects—primarily all natural objects, later all aesthetic objects—as if they were human, not from a tendency to treat them as if they were one's self. This innate tendency to characterize aesthetic objects has, I believe, a psychological basis broadly similar to (though at a higher 'level' than) that which is responsible for certain forms of synaesthesia; these forms we shall now examine.

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[1] *Ibid.* 443 ff.

[2] This *Journal*, 1912, v. 87–118.

4. *The Associative Aspect.*

(*a*) Synaesthesia. The subject of synaesthesia may be appropriately considered under the associative aspect, to which as we shall show in certain forms, it bears a close relation.

Of the twenty-nine subjects investigated, one, referred to as *M*, proves to be a remarkable case of coloured hearing. His condition has been already described in detail in a separate communication[1]. Certain general features may be repeated here. The colours which sounds evoke in him vary with the pitch of the tones, the highest audible notes being colourless, and the colours changing to green, greenish blue, blue, pink, orange and brown as the pitch is lowered. The colours vary slightly according as a given tone has been preceded by a higher or by a lower tone; they are also dependent on the timbre of the tone, a tone rich in overtones yielding a higher colour than one of the same pitch but poor in overtones. The two colours can usually be distinguished when a bichord is given; sometimes, however, the subject replies "a medley of colours," "other colours playing around," "a muddy mixture," "a complete mixture of colours," etc. The colours appear quite definitely to the subject at concerts (especially at orchestral concerts), but he usually suppresses them. Individual voices are differently coloured for him, but words evoke no colours. The sounds do not produce 'images' of colour. He is convinced that he does not 'see' the colours unless the sounds recall a coloured object, *e.g.* "the sky after sunset," a "B.A. hood," "a pink finger bowl," as occasionally happens.

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[1] This *Journal*, 1911, IV. 228—238.

Five other subjects, *H*, *L*, *O*, *Y* and *Z* give evidence of coloured hearing in a slighter degree.

To *Z* high notes appear as if dark and reddish, low notes as grey; he compares the first of the bichords $\frac{400}{700}\,\frac{1000}{1200}$ to 'grey,' the second to "a darker grey, glowing like a cinder"; the tone 1300 is like a "pure red." "I have never noticed these resemblances to colour before," he said, "they must come when I have no other thoughts or associations, but attend merely to the tones as such." He says the colours are *like* the sounds; they have the same effect upon him as the sounds.

Subject *L*, on hearing the tone 700 after a preceding 900, remarks "like orange is to yellow." The same subject, upon being given the bichord $\frac{800}{1000}$, says that it "wants more body or colour"; when, immediately afterwards, he is given the bichord $\frac{600}{800}$ he replies "more purple in colour"; he insists that he has no visual images of these colours.

Coloured hearing is also shown by the subject *O*, who, when confronted with the pair of bichords $\frac{800}{1200}\,\frac{600}{1200}$, prefers the second "because it is mellower" and describes a ball of light grey colour, seen slightly to his right, as accompanying his idea of mellowness; and, when confronted with the pair $\frac{400}{700}\,\frac{600}{1000}$, prefers the second for its combination (cf. p. 84) and obtains with it a faint image of a fine pink colour. The same subject with the bichord $\frac{600}{1200}$ obtains a "light salmon pink colour, a rich colour such as Burne-Jones used—a clear visual image without limitation of field[1]." With the tone 800, *O* sees a narrow stream of grey colour in mid-air which seems "to rise obliquely as the pitch seems to rise. There is nothing material about the light."

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[1] Curiously enough, *M* describes the tone 600 as "a rich dark Prussian blue, suggesting pictures—a Burne-Jones blue." Cf. also *Y* above for the bichord $\frac{600}{1200}$.

Subject *H* describes the bichord $\frac{400}{600}$ as purple and maroon, which are "like the lights one feels rather than sees, as in shutting the eyes in a bright light or in pressing on the closed eyelids. There is no outline or demarcation between the purple and maroon. They seem to fill the entire field."

Subject *Y*, on hearing the bichords $\frac{400}{700}\,\frac{800}{1000}$, describes the first as "dark," the second as "bright and yellow"; and on hearing the bichords $\frac{1000}{1200}\,\frac{600}{1200}$, he describes the first as "light," the second as "a dark steel colour." The second of the two bichords $\frac{500}{800}\,\frac{600}{1200}$ produces in him "the visual image of a deep blue spot with a bright centre which radiates downwards."

These are the only replies in which colour plays any part. From an examination of them it would appear that colour may arise (i) as a *sensory attribute* of sounds, not involving visual imagery (cf. subject *M*), (ii) as an *analogy*, experiences of colour being regarded as similar in form to those of sound (cf. subjects *Z* and *L*), (iii) as a *visual image* of colour either filling the visual field or restricted to a coloured object (cf. subjects *H*, *O* and *Y*).

Of these three varieties the second and third are closely related to the symbolic associations to which reference will be made immediately. The first, on the other hand, would appear to be related to that group of answers in which sensory language, other than that of colour, is employed. It is a matter of common experience that sounds of different pitch appear to us as massive or light, round or pointed, hard or thin, without involving any corresponding imagery. For most of us auditory sensations are more prone to evoke such tactual or tactuo-spatial attributes than visual or other sensory attributes. But with his coloured hearing, *M* finds the attribute of blueness to "come more easily," to be "more distinct" than, say, that of thinness of sound. On one occasion he describes the tone 700 as suggesting "a thick soup taste"; other subjects occasionally return replies in similar gustatory terms, *e.g.* for *D* and *K* respectively the tones 300 and 500 are "mellow," for *K* the tone 800 is "acid," for *I* the bichord $\frac{600}{800}$ is "insipid, like an alkaline taste." This suggests that what has been called 'tone character' (cf. p. 75) may reveal itself not merely in tactual but also in visual, gustatory or perhaps in other sensory terms, which do not necessarily involve a corresponding sensory impression or image.

(*b*) Symbolic associations. That the second and, especially, the third varieties of synaesthesia show a close relationship with symbolic associations, is indicated by the following illustrations:

O. $\frac{400}{700}$, $\frac{600}{1000}$. "Second pleasanter. With the first pair I had a visual image of a broad step and a small step beneath it [drawn]; with the second I had a faint image of a fine pink colour, rich but not glaring."

O. $\frac{1000}{1200}$ $\frac{500}{800}$. "Second pleasanter. In both cases there was an image of a line. The second sound was curved and gradually went into a straight line [drawn]; I followed this line, which itself was stationary."

O. $\frac{1100}{1200}$ $\frac{1200}{1300}$. "First pleasanter. I had the idea of being carried away, soaring in mid-air, into a region of mysticism. The second sound gave me the idea of lines crossing, and the notion of a clash."

E. 1100 1300. "The first has a big knock and a small tone. I visualised the former by a black disc about one inch in diameter, with a white dot placed 'at 5 o'clock' within the disc representing the latter. The second tone had the knock and sound of equal intensity; these I visualised as two equal overlapping discs, the black disc lying to the right of the white."

T. $\frac{300}{700}$ $\frac{500}{800}$. "With the first I saw two notes on paper, one above the other, thus :, with the second I saw them more widely apart."

O. $\frac{500}{800}$. "I had rather a feeling than an image of a big stream gushing forward from behind me and pushing me on; the primary factor was the image of my movement."

Z. $\frac{800}{900}$. "Very rich, entirely covering me, like waves coming to envelope me."

W. 1000. "A watery feeling like a lake; greenish."

W. 700. "It has the clearness of glass about it."

N. $\frac{600}{1200}$ $\frac{500}{800}$. "The first is grim and insistent. I saw a lot of spears upheld by people, and saw the glitter on the spears."

N. $\frac{1000}{1200}$. "A silvery sort of sound."

B. $\frac{300}{500}$ $\frac{500}{800}$. "With the second, I had a visual image of a tiny silver bell in the air before me; this probably arose from the thought of a silvery sound."

E. $\frac{700}{800}$ $\frac{800}{900}$. "With the first pair I saw two little brass balls, a little to the right of and above me, rubbing one another towards and from me."

Y. $\frac{600}{800}$ $\frac{600}{900}$. "With the second I saw two separate rods or fingers."

Y. $\frac{600}{1200}$ $\frac{500}{600}$. "I had the impression that the second contained three notes. I saw three tiny little balls, arranged in a straight line."

Z. $\frac{400}{700}$ $\frac{1000}{1200}$. "The first is big like a cylinder and moving quickly. The second is more like a bell, motionless but spreading out."

E. 800 1000. "The first appears as a cylinder, the second as a prism with its edge upwards."

N. 900. "Rather stupid, like a stupid parrot in the C— Zoological Gardens. I saw the parrot's head and beak,—nothing more,—looking 'at its silliest'."

A study of the introspective data shows that these symbolic associations involve the compresence of either the intra-subjective aspect or the character aspect, and a strong tendency to the associative. This conclusion is borne out by a study of the predominant aspects of these nine subjects *B*, *E*, *H*, *M*, *N*, *O*, *W*, *Y* and *Z*. *B* shows very strongly the intra-subjective and character aspects; she refers to feelings of strain, depression, hurry, and to feeling as if she were bending down, and she describes sounds as buzzing, piercing, thin, squeaky, soothing, comforting; she characterizes tones as sad, sedate, seeming to wail. *E* shows very similar features; he refers to sounds as upsetting him and drawing him off his balance; he mentions feeling as if he were toppling over, strange feelings of surprise, feelings of attraction and curiosity, thrills across his shoulders, and he describes sounds as soothing, grating, irritating, capable of being grasped as a unity; he characterizes tones as funny, lonely, etc. Subject *H* is also strongly of the intra-subjective type; she mentions feelings of laziness, surprise, excitement, interest, freedom, warmth in the ear, touches on the tympanic membrane, stinging up the arm, she describes sounds as tingling, aggravating, annoying, boring, irritating, disappointing; she also characterizes sounds occasionally as hoarse, crude, clumsy, *piquant*. So too, *O* is strongly of the intra-subjective type; he mentions impulses to bend low and to the right, as if reaching for a low object, and he refers to being carried to a quite different atmosphere; he also characterizes sounds occasionally, *e.g.* as thin-souled. Similarly, *Z* mentions feelings of tranquillity, sorrow, oppression, and robustness, and he describes sounds as rich, round, soft; he characterizes them as masculine, shallow, light-hearted, buoyant, and musical. Also, *T* mentions feelings of rest, expectation, doubt, and effort, he prefers low tones because he experiences greater strain (feeling a tendency to look upwards and to the right on hearing high notes), and he alludes to feelings of vastness when hearing tones of low pitch, which he associates with blueness and darkness. And *Y* speaks of sounds as warm, satisfying, or penetrating; and characterizes them as pretty, graceful, jolly, or sad. Subject *M* is strongly of the character type; he describes sounds as sympathetic, unsympathetic, congenial, unfortunate, gentle, etc. Subject *N* is also strongly of the character type; he characterizes sounds as trivial, grim, mysterious, stupid, silly, bony, bare. So too, *W* characterizes sounds as pretty, peaceful, etc. It is noteworthy that *O*, *T*, *W*, and *Y* all declare themselves to be specially impressed by the 'colour' of orchestral music.

(*c*) The Age of associations. In several cases the sounds awaken memories of childhood. For example:

a. $\frac{400}{500}$. "It reminded me of the opening notes of a musical box which I had when a boy."

J. 500. "I felt the buzz of a fork which I held in the fingers of my right hand, which took me back to an event in my childhood—something connected with playing in the life-time of my mother, who died when I was quite young."

J. $\frac{400}{700}$. "I felt a Jew's harp in my teeth and still have the taste of the steel in my mouth. I have not seen a Jew's harp for forty-five years."

R. $\frac{800}{1200}$. "Reminded me of the horrible sound we used to make as boys sticking nibs into our desks at school. I felt myself back in the schoolroom and I felt the prefect behind me."

T. 700. "Like the twang of the teeth of a musical box. I could see an old musical box with several teeth missing, which I played with 30 years ago[1]."

VI. COMPARISON OF THE ATTITUDES TOWARDS COLOURS AND SOUNDS.

A general comparison of the results of these experiments with those conducted by Bullough with colours shows that the former differ from the latter in two important particulars, namely (*a*) in the (even) greater rarity of 'pure types' and (*b*) in the greater frequency of the 'objective' type, pure and mixed with other types[2]. These differences are partly to be explained by differences of procedure. Bullough was occupied solely with the determination of that aspect or attitude which evoked acceptance or rejection. His object was to find out why a colour was liked; and the replies he obtained could be classified under four heads according as the colour was liked; (*a*) because it conformed with a subject's notion of what a good colour should be ('objective' type), (*b*) because of the sensory and other effects which the colour produced on the subject ('physiological' type), (*c*) because of the human characters with which the subject endowed the colour ('character' type), or (*d*) because of the associations which were suggested to the subject by the colour ('association' type). On the other hand, the purpose of the experiments described in this paper was to investigate the entire and spontaneous attitude of the subject towards musical sounds, and not merely to ascertain the attitude which he would adopt in order to pronounce an *aesthetic* judgment upon them. Consequently, the attitudes became at once more natural and more varied. Associations, for example, were apt to arise which would tend to be suppressed in Bullough's method of procedure, but which in these experiments may actually have affected the aesthetic judgment given.

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[1] *F* stated that when playing on the pianoforte he has often had vivid visual images of places which he has long forgotten but others have been able to explain to him. In listening to music, however, he has no visual imagery.

[2] The objective type is the most numerous. 41 °/o of judgments belonged to it. See later, Section X. [C. W. V.]

The differences obtained by Bullough's and the present methods of procedure are exemplified by the following illustrations from four subjects who were each investigated in these experiments with sounds, first by the one and later by the other method (in either order). The data obtained by Bullough's method (the subject being asked whether he liked it or not, and why) are given in the first column; those obtained by the method used generally in these experiments (the subject being asked to describe what came into his mind, how he was affected etc. by the sound) are given in the second column.

Subject V.

1200.	"Pleasant. I see no reason to dislike such a sound."	"Associated with one of Brahms's *Intermezzi*; it has a ring of that kind. I saw the page of music on which it was written. Pleasant."
300.	"Very pleasant. A richer sound."	"I wondered if it would be a low tone and suggest the C— chapel bell. Pleasant."

Subject B.

400.	"Pleasant for its softness and roundness."	"Strained feeling in trying to follow it from right to left upwards; I felt my entire self being raised. Indifferent."
1100.	"Unpleasant. Wailing."	"I saw myself going up a mountain pass, with distinct whistle of the train in which I sat. I heard the sound of the train first and then the visual image came. Pleasant because soft and far away."

Subject W.

$\frac{400}{700}$.	"Slightly pleasant. A blurred oriental feeling about it."	"I saw someone (not myself) knocking against a jar. Pleasant."
$\frac{500}{800}$.	"Pleasant. A pretty note, rather agreeable."	"Rather like a clock. Indifferent."

Subject X.

$\frac{500}{600}$.	"Unpleasant. An element of discord."	"Mystic. Vague thoughts of the sound coming from a cavern, whence arose a definite visual image of the rock-work in B— Aquarium. Pleasant."
$\frac{400}{700}$.	"Pleasant. Purely sensuous."	"The idea of a lyre came into my mind, then the idea of a woman, without imagery. Pleasant."

$\frac{1100}{1200}$.	"Unpleasant. Not full, something abortive, a failure."	"Something of a failure. Irritating, yet did not last long enough to cause actual irritation. Unpleasant."

These examples show how the effects of the objective and character aspects, obtainable in a state of relative purity by Bullough's method, are complicated by the intrusion of associative and intra-subjective aspects when the subject is allowed greater freedom of attitude.

Another reason for the rare occurrence of pure types in these experiments lies in the difference of material. A given tone or a bichord affects the average person more than a given field of colour or combination of colours,—perhaps partly because the former depends on vibrations of the air, the latter on vibrations of the ether. The emotions aroused by a painted picture are, as a rule, dynamically feeble compared with those aroused by a piece of music,—perhaps partly because the components in the former are presented simultaneously, in the latter successively, and hence with greater dynamic potency. It is in consequence not surprising that the physiological sub-aspects should intrude and commingle with the other aspects; indeed the only wonder is that such simple, relatively empty, material as isolated tones or bichords, should resemble music so closely in their actions.

The conative sub-aspect *b* also becomes more prominent in the case of sounds,—from a wish to discover their source and use. Here again sounds differ considerably from colours. No sooner do we hear a sound than we are prompted to determine whence it has come, what value or meaning it has or can have for us, whereas we are free to gaze on a field of colour without troubling ourselves appreciably about such matters. A sound of which we do not know the meaning is apt to torment us; hence, no doubt, the mystery attaching to curious sounds among primitive societies, and the survival of secret pass-words in more advanced secret societies of to-day[1]. In the case of colours, on the other hand, we do not so immediately seek to determine the meaning of the colour or the object to which it belongs. We are content to regard the colour as colour *per se* and to submit ourselves merely to its physiological or suggestive influence, or we may forthwith endow it with human characters.

One reason for the greater prominence of the objective attitude in hearing sounds than in seeing colours is that people are more often familiar with the technical or professional use of sounds than they are with that of colours. Not only may the subject be impelled actively to use the sounds, but he may also regard them objectively as suitable or unsuitable for a certain function. It is not surprising, then, that the objective aspect should influence the aesthetic judgment so much more in regard to sounds than in regard to colours.

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[1] Cf. the bull-roarer throughout a great part of the uncivilised world, and the great secret of the Melanesian secret societies which consists in learning how to emit a certain sound from a mysterious instrument.

A second, and very different, reason for the objective aspect consists in the absence of any personal relation of the subject to the sound. An insensitive, unmusical subject cannot regard it from the intra-subjective, character or associative aspects. He is forced, as a *pis aller*, to adopt the objective aspect.

On the whole, however, despite these various differences, there are well-marked similarities in the attitude to sounds and colours. Fortunately, fourteen of the subjects of Bullough's experiments were available for these experiments with sounds. As a rule, a general agreement is to be found in the type to which the subject belonged in the two classes of experiment. There are, however, several (five) cases of partial disagreement which deserve brief consideration.

One subject, *K*, who, for colours, is strictly of the character type and showed no associations, proves very strongly of the associative type for tones. To this subject a colour is almost a human being and is endowed with such personal attributes as morose, cheerful, insincere, serious, playful, etc. Tones, on the other hand, more often tend to suggest the instruments producing them, and vivid images of the scenes in which these instruments are sounding; as is shown by the following excerpts from her record:

500. "Deep, mellow, like a church note. I think I saw the inside of a church, it seemed to awake the feeling and dimness of a church interior. Pleasant."

300. "Like a foghorn, suggesting the sea...I at once saw the sea at F— with a haze over it. I felt (but did not see) myself standing on a certain spot on the parade. Pleasant in tone, but sad and discomforting for the association."

900. "A toy piano appeared. I saw it and particularly an end-note at the extreme right. Quite pleasant, but thin."

$\frac{500}{800}$. "I saw a certain room with F— at the instrument just starting to play with his right hand. Very pleasant."

$\frac{800}{1200}$. "I saw a woman in evening dress as if she were singing. Pleasant."

$\frac{400}{700}$. "It does not seem to harmonize; crude. Very unpleasant."

$\frac{600}{1200}$. "It recalls something, but I cannot say what. I felt something opening, something big coming out, of which I could not get hold. Then came a motor car, and finally the sea. Very pleasant."

$\frac{600}{800}$. "I vaguely saw an organ in church. Deep and full tone. Pleasant."

$\frac{800}{900}$. "It goes through the head; 'tinkettley.' Unpleasant."

It will be noted that although traces of the character aspect, which in the colour experiments was so predominant, are still evident, other aspects, especially the associative, have almost wholly replaced it.

Another subject, who differs considerably in his attitude towards colours and sounds, is unquestionably more sensitive to the effects of the former than to those of the latter. Consequently he proves to be markedly of the physiological type for colours and to be strongly of the objective type for sounds. Three other cases of partial disagreement are doubtless due to the already mentioned greater prominence of the objective aspect in the sound experiments. Two of these three subjects are not very musical, and there is good reason to believe that the third is less sensitive to sounds than to colours. They all therefore tend, *faute de mieux,* to regard the tones as independent objects rather than as affecting their 'feeling' or will.

VII. COMPARISON OF THE ATTITUDES TOWARDS SINGLE TONES AND SINGLE BICHORDS.

In order to compare the respective influences of single tones and bichords on the attitudes adopted by the subjects, the replies have been classified under the four aspects and the percentage frequency has been calculated for each aspect in the case of the two different kinds of material. Not infrequently, of course, a given sound evoked in a subject two (or occasionally even three) aspects, in which case all three are reckoned in the calculation. The following table gives the results :

	Intra-subjective	Objective	Character	Associative
Single tones	24	35	13	28 per cent.
,, bichords[1]	17	47	8	28 ,,

It is clear that in the case of bichords the objective aspect tends to increase in frequency at the expense of the intra-subjective and character aspects, whereas the associative aspect is as often evoked by bichords as by tones.

The percentage figures given above are here analysed in order to compare the results for the highly musical with those for the less musical. The seven most musical subjects have been separated from

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[1] The figures given by Dr Valentine for judgments on bichords (table, p. 105) yield the following percentage frequencies, which are remarkably close to the above:

Intra-subjective	Objective	Character	Associative
23	41	10	26

the rest, four of the seven being professional musicians; these constitute the 'highly musical' group, the remainder the 'less musical' group.

		Intra-subjective	Objective	Character	Associative
Single tones	highly musical	28	39	15	18 per cent.
	less musical	22	32	13	33 ,,
,, bichords	highly musical	26	49	4	21 ,,
	less musical	12	46	10	32 ,,

The above differences deserve examination, in the first place for each aspect taken separately.

(*a*) Intra-subjective aspect. This occurs more frequently among the highly musical, especially in the case of bichords. Now it will be remembered that the intra-subjective aspect involves not only (i) the physiological sub-aspects, evoking from the subject such replies as strident, piercing, going through the head, or causing repose, laziness, surprise, etc., but also (ii) the conative sub-aspect, which includes impulses to motion, efforts to determine the meaning, suggestion, use, pitch or interval of the sound. In only one subject (the only subject of the experiments who approaches very closely to a condition of absolute amusia) are these sub-aspects absent. Among the rest, there is not any appreciable difference between the highly and less musical subjects as regards the physiological sub-aspects. The most striking difference concerns the conative sub-aspect *b*, the highly musical subjects endeavouring to fix the pitch or the interval of the sound. As the ability to determine absolute pitch is less common than the ability to determine intervals, this difference in behaviour becomes much more marked for bichords than for single tones. The highly musical subjects declare themselves "worried to get the interval," "trying hard to get the interval," etc. In both groups, as has been already pointed out, the intra-subjective aspect is less evident for bichords than for single tones, especially among the less musical, there being a greater tendency for them to regard the bichords as objectively harmonious, discordant, complete, etc., or as having an objective quality of niceness, nastiness, etc.

(*b*) Objective aspect. Clearly, the just-mentioned tendency of the less musical to treat bichords from the objective aspect together with the tendency of the musical to give names to (*i.e.* to adopt a professional attitude towards) the intervals is responsible for the greater frequency of the objective aspect in bichords as compared with single tones.

(*c*) Character aspect. Alike among the highly musical and among the less musical sub-groups of subjects, the character aspect occurs less frequently for bichords than for single tones. This difference, however, is much more marked among the former than among the latter group; that is to say, it is especially from the highly musical subjects that the character aspect is evoked with much greater rarity by bichords than by tones. Probably the tendency of such subjects (to which attention has been drawn above) to regard bichords as musically useful objects suppresses the character aspect which is freer to develop in the case of single tones.

(*d*) Associative aspect. The number of associations evoked by the single tones and by bichords is not appreciably different in either group of subjects. But for both kinds of sound (and especially for the single tones), the less musical show a considerably greater number of associations than the highly musical. The associative aspect would naturally be encouraged among the less musical owing to their relative weakness in regard to the other three aspects. The highly musical tend more readily to act on a sound, to strive to determine its use, pitch or interval; to regard it as having use, or as being a certain note or interval. It is not surprising that among the less musical these tendencies should not be so well-marked, and that the suggestions roused through association should be freer and readier than among the highly musical.

VIII. COMPARISON OF THE ATTITUDES TOWARDS SINGLE BICHORDS AND PAIRS OF BICHORDS.

There remains for consideration the series of experiments in which the subjects were asked to describe their attitude towards two successive bichords, to decide which of the two (if either) they preferred, and to give their reasons for such preference. The reasons for their preferences will be discussed later. Here account will only be taken of their general attitude towards the tones and of the different aspects which the several subjects reported. The percentage frequencies of these aspects are:

	Intra-subjective	Objective	Character	Associative
Pairs of bichords	23	56	7	14
[Single bichords	17	47	8	28]

The most striking feature of the effect of giving a pair of bichords and asking for a judgment of preference,—as compared with the effect of giving a single bichord and asking for an absolute aesthetic judgment,—is seen to consist in (i) a large reduction in the frequency of associations, (ii) an increase of the objective aspect, and (iii) an increase of the intra-subjective aspect.

The following extracts from the reports of subjects allude to the paucity of association in preference judgments:

K. "There is not half so much imagery in comparing, because the mind is waiting for the second sound and is not given full play."

P. "I am not getting in the pairs the associations or the imagery which I had when the sounds were given singly."

Z. "The imagery I had previously is now fading, as I am now trying to discriminate between pairs of sounds."

If now the subjects be divided, as before, into a smaller 'highly musical' and a larger 'less musical' group, differences between the two groups are again revealed. The percentage frequencies for the various aspects are:

	Intra-subjective	Objective	Character	Associative
Highly musical	19	74	1	6
Less musical	25	48	17	10

The percentage frequencies of aspects among the highly musical for single tones, single bichords, and for pairs of bichords, are therefore:

	Intra-subjective	Objective	Character	Associative
Single tones	28	39	15	18
,, bichords	26	49	4	21
Pairs of bichords	19	74	1	6

That is to say, the same differences in the first three aspects, revealed by comparing their behaviour to isolated tones with that to single bichords, are carried still further when, in place of single bichords, pairs of bichords are substituted; there is a still further reduction in the frequency of the intra-subjective and character aspects, and a still further, indeed an enormous, increase in the preponderance of the objective aspect[1]. Nor is this surprising. The highly musical subject, when asked which of the two bichords he prefers, may be expected at once to revert predominantly to the objective aspect. When he has to give a judgment of preference, sounds lose their suggestive, physiological, and conative potency; they are regarded merely as independent objects, one bichord being preferred to the other "because it fuses better," "because it is less dissonant," etc.

In the less musical group, on the other hand, as might be expected, while there is the same enormous reduction in the frequency of the associative aspect, the frequency of the objective aspect does not materially increase:

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[1] Cf. Bullough's results with colours, This *Journal*, II. 411, 412.

	Intra-subjective	Objective	Character	Associative
Single tones	22	32	13	33
,, bichords	12	46	10	32
Pairs of bichords	25	48	17	10

Compared with the absolute judgments for single tones, the preference judgments towards pairs of bichords show, of course, a considerably greater frequency of the objective aspect, but this increase is hardly different for single bichords than for pairs of bichords. On the other hand, preference judgments tend to provoke from the less musical an increase in the frequency of the intra-subjective and character aspects. When asked which of the two bichords he prefers, he has not the same technical knowledge as the more highly musical subject. He cannot objectify the material, preferring one bichord "because it is a good harmony," etc. The utmost aid he can derive from the objective attitude will result in such judgments as "I like this bichord because it is fuller, richer, purer, etc." The less musical subject is consequently thrown back on the physiological aspect—preferring one of the bichords because it is "more satisfying," "warmer," "more soothing," or because the other is "irritating," "jarring," "setting the teeth on edge"; or, if he tends to characterization, he weighs the bichords from their character aspects, conceiving one or other of the bichords as "successful," "mystical," "inspired," "having ecstatic abandonment," "alive," "unsympathetic," "unfortunate," "melancholic," "miserable," "futile," "nasty," etc.

IX. RELATION BETWEEN ASPECT AND AESTHETIC APPRECIATION.

So far we have been considering the various aspects adopted by each subject in reference to their sounds, regardless of whether these aspects influenced the subject in his liking, indifference, disliking, or in his preference or equivalence. In the following table only those aspects have been taken into account which appear to have actually determined these reports. Each subject's data have been carefully scrutinized with this end in view, and the relevant aspects adopted by him have been arranged in order of frequency. If a subject's verdicts are based throughout on only one aspect, this naturally is given 'first place.' Generally, however, they are each based on two, or sometimes three, or even on four aspects; in this event, the most prominent aspect is given 'first place,' the next most important aspect is given 'second place,' and so on. The total number of times in which each aspect appears among all the subjects (twenty-three in number) is then calculated (appearing under the column headed 'No. of Times'); and similarly the total number of times each aspect takes first, second, third or fourth place for all the subjects is calculated (appearing under the columns headed 'Place').

Aspect (see pages 72–74)	Single Tones					Single Bichords					Pairs of Bichords				
	No. of Times	Place 1st	2nd	3rd	4th	No. of Times	Place 1st	2nd	3rd	4th	No. of Times	Place 1st	2nd	3rd	4th
I *a* i	14	11	3	0	0	11	9	1	1	0	12	8	2	2	0
I *a* ii	4	0	3	1	0	3	1	2	0	0	4	1	2	1	0
I *b*	7	2	3	1	1	9	2	6	1	0	10	3	4	2	1
I *c* i	1	0	0	0	1	1	1	0	0	0	3	1	1	1	0
I *c* ii	3	3	0	0	0	6	3	3	0	0	7	2	2	2	1
II *a* i	0	0	0	0	0	1	0	0	1	0	0	0	0	0	0
II *a* ii	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
II *a* iii	0	0	0	0	0	2	1	1	0	0	2	0	2	0	0
II *b*	11	5	4	1	1	9	5	4	0	0	17	6	8	3	0
III	7	2	1	4	0	6	1	2	3	0	6	1	2	2	1
IV *a*	1	1	0	0	0	1	0	1	0	0	1	1	0	0	0
IV *b*	0	0	0	0	0	1	0	0	1	0	0	0	0	0	0
IV *c*	1	0	0	0	1	4	0	0	2	2	1	1	0	0	0
IV *d*	1	0	1	0	0	1	0	1	0	0	0	0	0	0	0
	50	24	15	7	4	55	23	21	9	2	63	24	23	13	3

From this table we see that the average number of aspects made use of by each subject in order to arrive at an *absolute* appreciation in the case of single tones and bichords increases when he passes on to *preference* judgments in the case of pairs of bichords. The aspect which is chiefly responsible for this increase is II *b*, *i.e.* the aspect in which the subject considers the sounds in relation to his standard of purity, pitch, interval, etc., which a satisfactory tone or bichord should attain. It frequently appears in the preference judgments of subjects who had not employed it at all as a basis for their absolute judgments.

Both the experiments with single bichords and those with pairs of bichords show certain differences from those with single tones, which are worth noting. Aspects I *a* i and I *a* ii become less important, while aspects I *b* and I *c* ii become more important, in the former than in the latter. That is to say, in the case of single tones the subject's liking or disliking is determined more by their purely sensual effects upon him; while in the case of bichords it is determined rather by the changes which they prompt in his feeling attitude, and by the endeavours which they evoke to elicit their use or meaning.

The following aspects play no part in determining the absolute or relative appreciations of the seven highly musical subjects;—I *a* ii, in which tactile and organic sensations are aroused, II *a* i in which the sound is regarded as expressing language, II *a* ii in which the sound is treated as behaving like other non-musical sounds, and IV, in which associations are evoked. The above-mentioned prominence of II *b* in the preference judgments and of I *c* ii in the experiments with bichords[1] (singly and paired) is relatively more marked among the highly musical than among the less musical subjects. Among the sixteen less musical subjects, II *a* iii, in which the sound is regarded as having musical meaning, only occurs once.

These subjects who for single tones and bichords make predominant or sole use of sub-aspect I *a* i or I *b* (liking or disliking single tones and bichords because of their subjective effects of softness, mellowness, rigidity, stridency, etc., or because of the feelings of satisfaction, restfulness, aggravation, etc., evoked) give the following percentages of 'pleasant,' 'indifferent' and 'unpleasant' answers:

	Pleasant	Indifferent	Unpleasant
Sub-aspect I *a* i	71	8	21
,, I *b*	68	21	11

On the other hand, those subjects who for single tones and bichords make predominant or sole use of sub-aspects I *a* ii or II *b* (liking or disliking tones and bichords because of the organic associations aroused or because the sounds attained, or failed to attain, an objective standard) give the following percentages of answers:

	Pleasant	Indifferent	Unpleasant
Sub-aspect I *a* ii	17	0	83
,, II *b*	27	9	64

That is to say, the adoption of sub-aspect I *a* i or I *b* is accompanied by a predominance of 'pleasant' answers, whereas the adoption of sub-aspect I *a* ii or II *b* is accompanied by a predominance of 'unpleasant' answers.

The same facts are shown by the converse procedure of analysing the aspects adopted (*a*) by those subjects who give a predominant number of 'pleasant' answers, and (*b*) by those subjects who give a predominant number of 'unpleasant' answers for single tones and bichords. Among the former group the sub-aspects I *a* i and I *b* (and as a 'good third' II *b*) are those who occur most frequently; while among the latter the sub-aspects I *a* ii (in the case of bichords) and II *b* (in the case of tones) occur most frequently.

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[1] The close connexion between II *b* and I *c* ii has been already noted (cf. pp. 77, 79 footnote).

When reasons are given for an 'indifferent' answer, they can generally be traced either to too weak a reaction following from the intra-subjective aspect or to the adoption of the objective aspect. Such replies as there is "no body in it," "not much quality," "a metallic quality," "it was too shrill," show that the tone or bichord has not reached the subject's standard to which a satisfactory sound should attain.

The other aspects or sub-aspects, *e.g.* the conative sub-aspects of the intra-subjective aspect (I *c*), the character aspect (III), and the associative aspect (IV) appear to play no part in determining the relative frequency of 'pleasant,' 'indifferent,' or 'unpleasant' answers.

The percentage frequencies of the 'pleasant,' 'indifferent,' and 'unpleasant' answers, where the subjects can give an explanation of their verdict, are as follow:

	Pleasant	Indifferent	Unpleasant
Single tones	56	19	25
,, bichords	56	6	38

Hence the only differences as we pass from tones to bichords appear to consist in a reduction in the frequency of explicable 'indifferent' answers, and in a corresponding increase in the frequency of explicable 'unpleasant' answers. But as there seems no reason why the explicable 'indifferent' answers given to tones should not be equally divided, in reduction, between 'pleasant' and 'unpleasant' answers, we may perhaps conclude that the single tones tend, more frequently than the single bichords, to give rise to 'pleasant' answers and, less frequently, to give rise to 'unpleasant' answers. Doubtless this is easily explicable on the ground that the one great determinant of dislike of a bichord—its want of fusion, its beats, etc.—is absent in a single tone, and that the objective aspect is more frequently employed for bichords than for tones. (Cf. pages 92–94.) The greater number of 'indifferent' answers given by tones is no doubt to be explained by their relative meaninglessness.

In trying to arrive at a preference for one of a pair of bichords, several courses are open to the subject. (I) He may favour the one or the other of the bichords (*a*) because the one is liked, (*b*) because the other is disliked, (*c*) because the one is liked and the other is disliked, (*d*) because both are liked, one more (or less) than the other, (*e*) because both are disliked, one more (or less) than the other. (II) He may decide that the two bichords are equally liked or disliked. (III) He may find that a comparison between them is impossible.

(I) In by far the majority of cases, the subject's preference is based on (*a*) the positive liking of one of the bichords. There are more than twice as many replies of this kind, compared with (*b*) when the preference is ascribed to a positive dislike of one of the bichords. Here, as in the absolute judgments (cf. pages 96, 97), the sub-aspects which predominate in experiences of displeasure are I*a* ii and II*b*, whereas those which predominate in experiences of pleasure are I*a* i and I*b*. There seems to be little or no difference in the character and association aspects (III and IV) in the two cases. These facts are shown in the following table, where the figures are given in percentage values and the vertical columns refer respectively (*A*) to all the preferences calculated for all the subjects, (*B*) to those preferences which are based on a positive disliking of one of the bichords calculated for all the subjects, and (*C*) to those preferences based on a positive disliking of one of the bichords calculated for those five subjects who give the largest number of such judgments.

Aspect	*A*	*B*	*C*
I *a* i	23	7	5
I *a* ii	8	13	10
I *b*	20	15	5
II *b*	33	47	62
III	12	13	13
IV	4	5	5

(III) An analysis of the cases where the subject is unable to give a definite preference for one or other of the bichords, shows that this inability arises in the majority of cases from a conflict between the dictates of one or more attitudes. Thus, we get such replies as "I should want to use one of the sounds for one thing, the other for another," "the first is deeper, the second more buoyant, preference impossible," "I cannot decide between the pleasure of the thrill caused by the one, and the beauty due to the richness, low pitch, and fusion of the other," "I like them equally but with very different feelings."

Another obstacle to comparison, due, like the above, especially to the intra-subjective aspect, is attributable to the effect produced on the subject by the transition from the one bichord to the other. Thus for the pair $\frac{600}{800}\,\frac{1000}{1200}$ *E* describes a feeling of "shifting as if toppling over a raised place between the two sounds, one placed on the left, the other slightly higher on the right, a slight thrill being felt across the shoulders in passing from one to the other." Similarly with $\frac{1000}{1300}\,\frac{500}{700}$, he feels one on each side of him, "the higher on the right, one clashing with the other, and breathlessly expects a new sound to arise when they meet." So, too, *X* reports of the bichords $\frac{500}{800}\,\frac{600}{800}$ that "only the idea of ascent from the first makes the second sound the more pleasing," and of the bichords $\frac{600}{1000}\,\frac{800}{1200}$ that "the idea of development and movement towards something higher, something attained, makes the second preferable," but that "apart from this, the first would be the more pleasing."

Less commonly the sole use of the objective aspect is responsible for the inability to prefer one of the bichords to the other, the subject tending to regard them as making up an objective whole. For example, "they belong together as parts of a melody, and so I cannot make a preference," "they both fit together, comparison impossible," "I cannot keep them apart";—or conversely, "they lie quite apart from one another, and have no meaning, so I have no preference," "they are not comparable, as they do not form a whole." Occasionally, neither bichord attains a satisfactory quality, and a preference becomes impossible because the qualities are not the same, as for example "the first is impure, the second though purer is too hard."

Three subjects *P*, *F*, *G* complain of the difficulty of remembering both sounds for the purpose of comparison: "The second is apt to destroy the memory of the first," "the second knocks out and upsets the first," "I forget the second because, if it comes along while the first is still sounding in my ear, I fail to hear it."

X. RELATION BETWEEN ASPECT AND AESTHETIC APPRECIATION (*continued*) [by C. W. VALENTINE].

It is characteristic of the aesthetic attitude that it is essentially objective and disinterested; self is 'lost' in the beautiful object. Attention is completely concentrated upon it. From this point one must class the intra-subjective aspect[1] in these experiments as very low on the aesthetic scale, perhaps lowest (following Bullough), though in some cases the intra-subjective aspect may be adopted only when the aesthetic experience proper is over, and introspection begins.

From a similar point of view a low aesthetic value characterizes the non-musical associative judgments (where the interval suggests an object which might have given rise to a similar sound, *e.g.* a gong, a clock striking). Attention is not concentrated upon the musical interval, and the transition of thought to the associated object is obvious. Such cases may be compared to the badly fused associations in Bullough's experiments with colours.

From this same point of view a higher type of aesthetic judgment is found where there are musical associations proper, which imply some knowledge of or familiarity with music, and sometimes a memory of a definite musical composition.

So far we have taken as a criterion of the aesthetic value of a mental attitude its objectivity or 'autotelicity[2],' the extent to which the attention is concentrated upon the aesthetic object itself rather than upon non-fused associations or upon the self.

But another criterion is possible, namely the degree of pleasure accompanying the perception of the object presented. No implication is here made that pleasureableness and aesthetic value are synonymous. But even if there may be aesthetic value without *any* pleasure (which would seem doubtful) it is still probably true that when judgments are given upon a series of comparatively simple objects, such as musical intervals, there is likely to be a distinct correlation between the order of the degree of pleasingness of the experiences produced by the intervals, and the order of the aesthetic value of these experiences.

Accordingly in the following table the various aspects have been arranged according to the proportionate frequency with which each aspect coincided with a high degree of pleasure (as indicated by the judgment "very pleasing"). Parallel to this is given Bullough's order of the aesthetic value of the aspects in the case of colours, based upon the 'autotelicity' of these aspects.

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[1] It is worth noting that Dr Myers's use of this aspect is not quite the same as mine. In particular, his sub-aspect I *a* (i) includes a number of judgments which I classified as 'objective,' *e.g.* hard, thin, soft. The distinction is important in considering the hedonic and aesthetic value of this aspect.

[2] See Bullough, *op. cit.* p. 461.

	Judgment on Musical Intervals. Criterion—degree of pleasure		Judgments on Colours. Criterion—'autotelicity'
I.	Character (24·7 °/o).	I.	Character.
II.	Musical (fused) associations (24·5 °/o).	II.	Fused Associations.
III.	Objective (19·9 °/o).	III.	Objective.
IV.	Intra-subjective (14·9 °/o).	IV.	Non-fused Associations.
V.	Associations other than musical (non-fused) (12·5 °/o).	V.	Physiological.

In brackets are given the percentages of all judgments conforming to the aspect in question, which are also accompanied by the judgment "very pleasing[1]." The calculations are based upon the records of over 3000 judgments given by 146 subjects.

It will be seen that the orders in the two columns are the same except that Bullough places the physiological aspects fifth while in the 'musical' column the intra-subjective aspect takes the fourth place.

In his later paper on colour combinations[2], Bullough seems to give a higher place on the aesthetic scale to the physiological aspect. In a personal communication, however, he explains that he there refers to transition cases from the physiological to the character aspect; he would still put the purely physiological type at the bottom of the list. Such transition cases in the present musical experiments would almost invariably be classed as subjective; and these, together with the wider significance of the term 'intra-subjective' as compared with Bullough's term 'physiological,' may account for the priority of the intra-subjective aspect in regard to the non-fused associations in the scale of aesthetic values for the musical experiments.

A minor point of difference between the two orders may appear in the fact that in the list for musical intervals, the fused associations are bracketed (practically) first with the character judgments. But in reference to fused associations Bullough writes[3]: "If it were not for the inherent instability of the fusion and for the danger of the reappearance of the old cleavage between colour and associated content, this type might almost represent the highest form of aesthetic colour-appreciation."

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[1] In calculating the percentages for this table it seems much fairer to ignore the large number of displeasing objective and physiological judgments upon the four discords, with which naturally such remarks as "discord," "notes do not blend," "jars on the nerves," etc. are very frequent. The discords indeed seem to appear to many subjects as outside the sphere of aesthetic objects altogether. As one very unmusical subject said, as regards a discord, "It ought not to be in the piano."

[2] This *Journal*, 1910, III. 406–447.

[3] *Ibid.* 1908, II. 462.

Now in the case of the present experiments the musical associations were, as we have seen, often so intimately fused with the presentations of the given intervals as to be inseparable from them. Subjects frequently found that they could not hear and judge a musical interval by itself; a following and completing interval was intimately bound up with it and intensified the pleasure due to the given interval, or actually made pleasing an interval which, quite alone, would have been displeasing. Hence these fused association judgments may more frequently represent the highest form of aesthetic appreciation in the case of musical intervals than the corresponding judgments do with colours. It seems as if the fundamental nature of the types is emphasized by the fact that two such different criteria as objectivity and the degree of pleasure give such similar orders of aesthetic value[1].

XI. APPENDICES.

APPENDIX A. *Other Determinants of Aesthetic Appreciation.*

1. *Pitch.* Many of the subjects spontaneously observe that their liking of the sounds is dependent on their pitch. Nine of them prefer low tones,—"discords sound pleasanter when lower" (*C*), "lower sounds are more attractive" (*E*), "I prefer lower tones" (*G*), "second pleasanter owing to its being deeper" (*I*), "I like the deeper sounds" (*J*), "I prefer deeper tones because they are more suggestive" (*K*), "I prefer low tones for their fulness and richness" (*R*), "I like deep sounds" (*T*), "I generally like low tones for their fulness" (*V*).

Among these subjects, the conative sub-aspects (I *c* ii and to a less extent I *c* i), the associative (IV), and the objective (II) aspects are particularly prominent. Two subjects, on the other hand, prefer high tones, and both these adopt especially the physiological sub-aspects I *a* i and I *a* ii.

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[1] I may add that I have more recently also found this scale of aesthetic value of the aspects useful in dealing with judgments upon pictures and also upon poems. In these departments with a careful and thorough investigation it seems possible to form a fairly reliable estimate of the aesthetic development of an individual according to the extent to which he uses the higher or lower types of judgments.

In none of these eleven subjects is the character aspect predominant. It occurs in only two subjects of this group (*E* and *Z*); of whom the latter remarks "that very high sounds give me the feeling of sorrow and oppression; so do very low sounds, yet the sorrows of the latter are not so light-hearted, but are deeper and more intellectual,—like a person disappointed in love becoming a poet." We see, then, that low tones have a more powerful effect in evoking suggestion and motor and intellectual activity, and in satisfying the subject's objective standard; whereas the higher tones depend rather on their physiological effect on the subject. This is well borne out in the case of *D* in whom, throughout the sitting, there is always a conflict between the low tones which appeal to him for their rich beauty, and the higher tones which he likes for their sensual effects (cf. p. 98).

2. *Timbre.* Two subjects, both markedly of the physiological type, make mention of this criterion—"I like rich warm sounds" (*M*), "I like soft smooth sounds" (*F*). Occasionally the character aspect evokes such answers as *V*'s—"rather trifling; not a quality one would want in a musical composition."

3. *Familiarity and Strangeness.* Perhaps the most striking instance of the effects of familiarity and strangeness on appreciation is afforded by the highly musical subject *F*. It happened that he was presented with the same bichord $\frac{500}{700}$ several times in the course of the sitting. The following are the remarks which he made about it at different presentations.

> "A horrid interval, not harmonic. I couldn't tell what it was meant for. Grotesque. I didn't want to estimate it."
>
> "Unpleasant, fidgetty. I couldn't tell what it was meant for. 'What is that?' I feel impelled to ask."
>
> "Not so bad as that horrid thing" [referring to the same bichord].
>
> "I couldn't make it out."
>
> "I don't mind so much now, as I see what it is—a diminished fifth."

On the other hand, an attitude of less intense strangeness (towards precisely the same bichord) may lead to a positive appreciation,—as in the case of the more physiological, less conative subject *E*, who reports of the two bichords $\frac{500}{700}\,\frac{600}{800}$,—"I prefer the first for its odd agreeableness; it excited my curiosity to know if it were a familiar interval."

In four subjects, *B*, *C*, *M* and *P*, who make spontaneous mention of familiarity, there is present a well-marked associative tendency. This tendency is combined with the conative sub-aspect in the case of the first two subjects, and both of them complain that familiarity tends to reduce the pleasure of the bichords. In the case of the last two, on the other hand, it is combined with a more passive physiological sub-aspect, and in them familiarity conduced to a positive appreciation.

The objective aspect, as we might expect, makes familiarity and strangeness unfavourable to a positive appreciation, the interval being rejected because it is "commonplace," or of "surprising *Klangfarbe*."

4. *Association.* In many cases it is difficult to be sure that associations are directly responsible for the pleasure or displeasure produced; yet the following examples will suffice to show the exercise of such influence.

J. $\frac{400}{700}$. "Unpleasant because I felt a Jew's harp in my teeth and still have the feel of the steel in my mouth."

K. $\frac{600}{800}$ $\frac{500}{800}$. "I preferred the first because I saw a man singing in — College Chapel."

M. $\frac{600}{1200}$ $\frac{500}{800}$. "The first preferred. The second suddenly suggested a church bell being tolled and became definitely mournful."

Q. $\frac{800}{1000}$. "Pleasant, reminding one of the opening chord in the *Dead March from Saul*; I like this piece."

W. $\frac{400}{600}$ $\frac{600}{1000}$. "The first preferred because I thought of an association with a gong."

Z. 500. "Unpleasant, because it reminded me of the sound of an instrument which I don't care for."

5. *Prejudice against interval.* The objective attitude several times led the most highly musical subjects to base their judgments on standards which previous experience had set up. Thus *N* remarked of the bichord $\frac{600}{800}$ "it sounds bony, bare, angular, unclothed, as all fourths do," and of the pair $\frac{600}{800}$ $\frac{1000}{1200}$ "I prefer the second; fourths always strike me as a strong discord. I prefer the augmented to a perfect fourth."

On the other hand, for the same pair of bichords *U* alludes to the "curiously pleasant and interesting charm of novelty" of the first; "but I cannot prefer a fourth to a minor third." Indeed he confesses with the bichords $\frac{600}{1200}$ $\frac{500}{800}$,—"I am now biassed. Octaves are pleasanter than sixths." This subject's prejudicial attitude even leads him to reject an interval because "it is not appropriate for the *Glockenspiel*" (to the tones of which instrument he likens those he is hearing in the experiment).

6. *Conflict between sadness and pleasure.* The following examples illustrate the fact that a sound may be liked despite the mournfulness or sadness connected with it.

J. 500. "I had a feeling of desolation. Then I thought of a fog signal, and finally came the image of a Flushing crossing on a foggy summer morning. I was on deck, leaning over the rails. And yet I like the sound."

K. 300. "Like a fog horn suggesting the sea, quite of its own accord. I at once saw the sea with a haze over it....Pleasant in tone, but sad and discomforting for the association."

D. $\frac{300}{700}$. "Mournful but rather pleasant."

S. 500. "Rather sad, but I like it."

B. $\frac{600}{1000}$. "Very pleasant, though sad."

X. $\frac{600}{900}$. "Pleasant for its mellowness and purity. Yet it gave me a feeling of sadness and melancholy."

In all these subjects, the objective and character aspects are combined with one another or with the intra-subjective aspect. In many cases, at least, the answers appear to be due to a balancing or wavering between different aspects, the more 'distant' aspect, for example, giving pleasure despite the feeling of sadness produced in the subject, such subjective impressions tending to yield before the independent objectivity or character of the sound.

APPENDIX B. *Indications of Sexual and Racial Differences*

[by C. W. VALENTINE].

The men and women students who acted as subjects in my experiments were drawn pretty much from the same class of homes, the women, if anything, having had some advantage in the way of hearing music, and certainly more women seem to have had a considerable amount of instruction in the playing of the piano. At first it appeared that there were interesting differences between the men and women as shown by the following table.

Total number of judgments of different types.

Types of judgment	Men	Women
Character	137	79
Musical Association	140	139
Objective	512 (253)	409 (211)
Intra-subjective	256 (133)	273 (149)
Associations other than musical	119	191
	1164	1091

As the number of men was only three-quarters that of the women the totals of the men's judgments are increased proportionately in this table for the sake of easy comparison. The numbers in brackets are obtained if the judgments on the four discords are ignored.

It will be seen that the most striking differences are shown in the highest type (character) where the men surpass the women by 70 °/₀, and in the lowest type where the women surpass the men by nearly 70 °/₀. The women as a whole give a smaller number of reasons for their judgments, yet they give more than the men in the two types of lowest aesthetic value. Of the second highest type—that of musical associations—the women give as many judgments as the men, but this we should expect from their greater familiarity with musical compositions.

In general I have found women students much more thorough and facile than men in introspective work in other departments of aesthetics, for example with colours. Yet in these music tests fifteen women and only five men had to be classed as of neutral type, because of the poverty of their introspective remarks, and even among these the men average 1·2 character judgments and the women only 0·3. My general impression in reading over the introspective remarks of these one hundred and forty-six subjects was that there were more men than women who were deeply sensitive to the impressions of musical intervals.

So far we seem to have grounds for assuming a marked difference between men and women in the predominance of the various types of judgments. But a more careful analysis of the results suggested that the great superiority of the men in the number of character judgments may be indicative of a racial rather than a sex difference. There were nineteen English and twelve Welsh among the men students. These, taken together, give seventy-five character judgments against twenty-eight by exactly the same number of Scots[1]. Unfortunately when one splits up the numbers in this way, they become too small for any wide generalisation. But if we separate the various nationalities and average the judgments of the various types we have the following results.

[1] I am informed by the Director of the Training College that there is no special selective factor at work likely to result in the English and Welsh men being in any way superior to the Scots at the College.

	Character	Musical Assoc.	Objective	Intra-subjective	Other Assoc.
For 84 Scots (women) average number of judgments given by each subject	0·9	1·6	4·9	3·2	2·4
For 31 Scots (men) average number of judgments given by each subject	0·9	2·6	6·2	3·0	1·7
For 19 English (men) average number of judgments given by each subject	2·3	1·0	5·6	3·6	1·6
For 12 Welsh (men) average number of judgments given by each subject	2·5	0·5	7·1	2·5	0·5

On the basis of the given scale of aesthetic values of the types we should probably have to award first place to the Welshmen in regard to the aesthetic appreciation of harmonious intervals, a result not surprising to one who knows the average student of the three countries fairly well[1]. The Englishman would take second place. The results of the Scots men and women are more alike. But the men give more judgments of the second highest aesthetic type (Musical Association) and less of the lowest aesthetic type (Associations other than musical) than do the women.

We may conclude then that the apparent differences shown by the sexes in these experiments are probably, in part, racial differences, though not perhaps entirely so.

APPENDIX C. *Changes in Experience while Listening.*

1. *Affective Changes.* Several subjects, all employing physiological (I *a* and I *b*) and objective (II *b*) sub-aspects, report a change in their affective experience while listening to the sounds. Thus *Aa* remarks of the bichord $\frac{800}{1200}$, "I didn't like it at the beginning; it seemed thin and metallic, but it improved later, got richer in quality"; and of the bichord $\frac{700}{800}$, "it seemed to change its pitch as it went on. The first

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[1] I was able subsequently to test eight other Welshmen also, thus bringing the number of Welshmen to twenty. If their results are added to those of the other Welshmen we get the following average per man:

Character	Musical Associations	Objective	Intra-subjective	Other Associations
2·7	0·9	8·5	3·5	0·5

It should be noted that this last group had to be taken at the end of a hard day's teaching, when the subjects were unusually fatigued.

part I liked to some extent; the second I liked very much. I had a diagrammatic image of how it jumped,—a horizontal line running from left to right suddenly going up vertically." *B* describes the bichord $\frac{1100}{1200}$ as unpleasant at first, but becoming "pleasant as it dies away," and the bichord $\frac{800}{1000}$ as "not so pleasant later as at first, owing to its becoming confused." *O* finds the tone 1200 "pleasant at first but losing pleasure owing to its seeming to be a leading note, carrying a feeling of dissatisfaction and incompleteness"; he finds the tone 500 "very pleasant at first; the sound had body about it. Then it became unpleasant seeming to be below a note I would have liked better." *X* reports of the tone 400—"pleasant but I am disappointed at its abrupt ending"; and of the bichord $\frac{600}{1200}$ "at first unpleasant, later giving me the idea of something triumphant and stimulating me." Subject *I* thinks the tone 900 "unpleasant at first because it was thin, but later it gained in strength and confidence." *M* describes the bichord $\frac{500}{800}$ as "at first too loud for that particular quality of sound, but improving as it dies away."

Several subjects, all employing the physiological sub-aspect, report that the sound if continued for long would become unpleasant. Thus *P* says of the tone 1200 that "it would make me irritable if continued" and *Y* of the bichord $\frac{1200}{1300}$ that "it is on the way to becoming excruciating." So *G* and *W* describe the tone 800 as "approaching the kind of sound that makes the fingers tingle" and as "if it lasted much longer it must set the teeth on edge," while *Z* reports of the bichord. $\frac{500}{800}$ "I couldn't stand it for long; it would become too oppressive."

2. *Objective Changes.* *E* $\frac{500}{700}$ "As it continued, one of the two notes appeared to turn, the higher imaged to the right of the lower and appearing to move so that it lay virtually over the lower."

Aa 500. "A full round quality, richer than the other. While I was getting the quality I had an idea (? image) of a crescendo $<$ the lines not starting from a common point."

APPENDIX D. *The Nature of the Interval.*

1. *Order of preference for Intervals.* Inasmuch as the attitudes assumed by the different subjects in their preference judgments have proved so various, and as these experiments were planned only to analyse these varieties, no attempt will be made here to determine the average order of preference for the different intervals[1]. The only points worth noticing under this head are that the general choice is often, *inter alia*, affected (1) by the order in which the members of any pair of bichords are sounded, (2) by the pitch of the two intervals employed. For example, in regard to the latter point, whereas a lower minor third $\left(\frac{500}{600}\right)$ is always preferred to a higher octave $\left(\frac{600}{1200}\right)$, a higher minor third $\left(\frac{1000}{1200}\right)$ is often preferred to the same octave $\left(\frac{600}{1200}\right)$. On the other hand, while a minor seventh $\left(\frac{400}{700}\right)$ is generally preferred to a low major or minor third or major sixth $\left(\frac{400}{500}, \frac{500}{600}, \frac{300}{500}\right)$, the same intervals, when their tones are raised an octave, $\left(\frac{800}{1000}, \frac{1000}{1200}, \frac{600}{1000}\right)$, are more often preferred to the same minor seventh $\left(\frac{400}{700}\right)$. Again, the minor third $\left(\frac{500}{600}\right)$ is preferred to the fourth $\left(\frac{300}{400}\right)$, while the octave-higher fourth $\left(\frac{600}{800}\right)$ is more often preferred to the octave-higher minor third $\left(\frac{1000}{1200}\right)$.

Such differences are the natural outcome of the application of other criteria, in addition to that of fusion, in the formation of preferences for intervals. Thus, the bichord $\frac{400}{700}$ is by some subjects preferred to the bichord $\frac{600}{1000}$ because it is (*a*) richer, (*b*) of better quality, or (*c*) of lower pitch, because the other is (*d*) mournful or (*e*) unsympathetic. Others, of course, make the opposite choice, on the ground that the second bichord is (*a*) more musical, (*b*) more consonant, (*c*) fuses better, etc.

2. *Misjudgment of Interval.* It is curious how far even the most highly musical subjects go astray in their uncontrollable endeavours to find out what the intervals are. The tuning-fork tones are of so strange a timbre that, as one of the subjects remarks, "the recognition of these intervals is not so easy as it would be on the piano or violin." Another observed—"it is hard to tell such thin sounds. I cannot get away

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[1] For recent investigation into this matter, cf. Valentine, This *Journal*, 1913, VI. 190–216.

from the trouble of these notes." In some cases the poorness of timbre makes the intervals appear flat. Thus *Ab* reports of the single bichords $\frac{800}{1200}$ and $\frac{600}{800}$ that they are not quite in tune, the former being dull and flat; and *T* complains that $\frac{300}{400}$ is flat. In other cases the subjects replace one of the tones by its higher or lower octave; thus, *N* estimates the bichord $\frac{800}{1000}$ at one time as a major tenth $\left(\frac{800}{2000} \text{ or } \frac{400}{1000}\right)$, at another as a major sixth $\left(\frac{800}{500} \text{ or } \frac{1600}{1000}\right)$, and *V* the bichord $\frac{600}{1000}$ as a minor third $\left(\frac{1200}{1000} \text{ or } \frac{600}{500}\right)$! This bichord $\frac{600}{1000}$ is called by *N* an augmented fourth! Subjects *Ab, V* and *N* are all first-rate professional musicians. Yet *N* calls $\frac{1000}{1200}$ at one time a major third, at another time a perfect fifth!

XII. CONCLUSIONS.

1. The aspects which are employed by subjects towards sounds may be classified (i) as intra-subjective, (ii) objective, (iii) character, and (iv) associative. With the exception of (iii) these aspects are divisible into various important sub-aspects (pp. 72–74).

2. The character aspect is not dependent on the intra-subjective or associative aspects; in nature and origin it is *sui generis* (pp. 79–81).

3. Probably a close relation exists between certain forms of synaesthesia and symbolic associations (pp. 81–85).

4. Symbolic associations are commonest among those subjects in whom either the intra-subjective or the character aspect is combined with a strong tendency to the associative (pp. 84, 85).

5. While there is a general agreement between attitudes adopted by subjects to colours and to sounds (p. 89), certain important differences are revealed and to these attention has been drawn (pp. 86–90, 100–102).

6. The objective aspect occurs more frequently and the intra-subjective and character aspects occur less frequently in the case of bichords than in the case of single tones (pp. 90–92).

7. The associative aspect occurs most frequently among less musical subjects (p. 92).

8. Preference judgments reduce the frequency of the associative aspect and yield a much higher frequency of the objective aspect and a slightly higher frequency of the intra-subjective aspect (pp. 92–94).

9. The relative frequency with which the physiological and conative sub-aspects of the intra-subjective aspect influence the subject's appreciation, varies according as the material consists of single tones or of bichords (pp. 95, 96).

10. The objective aspect or too weak a result of the intra-subjective aspect is generally responsible for indifferent appreciation (p. 97).

11. Impossibility of comparison arises either from a conflict between the dictates of one or more aspects, or from reliance on the objective aspect (pp. 98, 99).

12. The physiological sub-aspects of the intra-subjective aspect more often yield positive appreciation, the conative sub-aspects and the objective aspect more often yield negative appreciation of the sounds (pp. 96, 97).

13. Low tones are more potent in evoking suggestions and intellectual activity; high tones depend rather on their physiological effect (pp. 102, 103).

14. The influence of familiarity on appreciation is most marked in subjects who make frequent use of the associative aspect, while that of strangeness is most marked in those who make frequent use of the conative sub-aspect and the objective aspect. Familiarity has a positive effect when the associative is combined with the conative sub-aspect, and a negative effect when the associative is combined with the physiological sub-aspect (pp. 103, 104).

TWO CASES OF SYNAESTHESIA.

BY CHARLES S. MYERS.

I.

DURING his recent visit to England, the well-known Russian composer, Alexander Scriabin, kindly allowed me to carry out an examination of his coloured hearing. Unfortunately the time at his disposal and the necessity of communicating in a language (French) foreign to both of us[1] made a very thorough investigation impossible. But the results obtained form perhaps a sufficiently interesting contribution to the psychology of music to merit publication in their present form.

Colours form for Scriabin so important a part of the total effect of sounds that he desires his *Prometheus* to be performed to the accompaniment of concealed lamps which shall flood the concert-hall with a light of ever-changing colour; the music of his *Mystery*, when completed, will be presented with a similar play of colours, and with odours.

Scriabin's attention was first seriously drawn to his coloured hearing owing to an experience at a concert in Paris, where, sitting next to his fellow-countryman and composer Rimsky Korsakov, he remarked that the piece to which they were listening (in D major) seemed to him yellow; whereupon his neighbour replied that to him, too, the colour seemed golden. Scriabin has since compared with his compatriot and with other musicians the colour effects of other keys, especially B, C major and F♯ major, and believes a general agreement to exist in this respect. He admits, however, that whereas to him the key of F♯ major appears violet, to Rimsky Korsakov it appears green; but this deviation he attributes to an accidental association with the colour of leaves and grass arising from the frequent use of this key for pastoral music. He also allows that there is some disagreement as to the colour-effect of the key of G major. Nevertheless, as is so universally the case with the subjects of synaesthesia, he believes that the particular colours which he obtains must be shared by all who are endowed with coloured hearing. Doubtless, on more systematic inquiry, the same disagreements which, as is well known, occur in the case of coloured vowel sounds, coloured names, coloured days of the week, and (as I have pointed out[1]) with coloured pitch and coloured timbre, will be discovered in the case of coloured tonality[2].

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[1] The latter difficulty was much reduced by the subsequent cooperation of M. Alexander Briantshaninov, to whom I gladly take this opportunity of expressing my obligation.

As will have been gathered by now, Scriabin's chromaesthesia refers to the *tonality* of the music. As the tonality changes in a piece, so the colour changes. Scriabin explains that "the colour *underlines* the tonality; it makes the tonality more evident." The colour or a change of colour sometimes appears to him before he is aware of the tonality or of a change of tonality. For such reasons he believes that the musical effects are enhanced by the simultaneous presentation to the eye of the appropriate colour.

In general, when listening to music, he has only a 'feeling' of colour; only in cases where the feeling is very intense does it pass over to give an 'image' of colour.

The older music, with its infrequent changes of tonality, gives him a colour changing in intensity instead of in quality; "it has not the psychological basis of modern music." Certain compositions, and most of Beethoven's Symphonies, are not of a kind to need colour; they are "too intellectual in character."

In the case of coloured hearing which I have previously published[3], the colour is dependent on the *pitch* of the note; in many other cases, already described, it is dependent on the *name* of the note. But for Scriabin a single note has in itself no colour; it has the colour of its tonality. Indeed, he insists that whether in or apart from musical compositions, a single tone cannot occur alone; even outside music it is accompanied by overtones, which, in many cases, especially in the case of the sounds of nature, include inharmonic overtones, in addition to the harmonic series. Thus it is, that if we imitate the sound of a bell by adding an appropriate series of weaker tones to a louder fundamental tone on the pianoforte, we may find on analysis a combination of two or more tonalities, and consequently an inter-play or fusion of the corresponding colours. So, too, any simultaneous combination of tones produces on Scriabin a simple or composite colour effect according as it suggests one or several tonalities.

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[1] "A Case of Synaesthesia," This *Journal*, 1911, IV. 228–238.

[2] Cf. Th. Flournoy, *Des phénomènes de synopsie*, Paris et Genève, 1893, 101; also Henri Laures, *Les Synesthésies*, Paris, 1908, 30.

[3] *Loc. cit.*

The strongest colours for Scriabin appear to be those relating to the keys of C major, D major, B major and F♯ major, placed respectively in the red, orange, yellow, blue and violet. Starting, however, from C at the red end of the spectrum, Scriabin finds that

Red Orange Yellow Green Blue Violet
C G D A E B F♯

as he passes from hue to hue, the successive colours correspond to tonalities rising by a series of fifths. Thus the key of C is red, of G red to orange-red, of D orange to yellow, of A yellow to green, of E green to blue, of B blue to violet, and of F♯ violet. The colours of the remaining keys D♭, A♭, E♭, B♭ and F are believed by Scriabin to be extra-spectral,—either ultra-violet or infra-red. Thus the key of F is "on the verge of red," giving often the effect of a metallic lustre.

Into Scriabin's objections to the terms major and minor in tonality it is impossible to enter here. The harmonies, in his *Prometheus*, are based upon a scale of six notes, which represent the seventh, eighth, ninth, tenth, twelfth and thirteenth overtones of a fundamental, yielding the tones (in the scale of C) *c d e f*♯ *a b*♭ approximately. A series of such tones, *e.g.* $c^1\ f^1\sharp\ b^1\flat\ e^2\ a^2\ d^3$, simultaneously played in true (untempered) intonation, produces what he terms a "single sound," one that is not easily analysable. Scriabin believes that there is always a psychical struggle between a tone and its overtones, just as a rivalry may be conceived between the various tones emitted at any moment by an orchestra,—a contest for the pitch of the single fundamental sound which the listener shall hear.

In my previous paper on synaesthesia I concluded that "for the *full* development of synaesthesia, a strong tendency to a certain kind of association is requisite—a tendency to form associations between corresponding members of two homologous series[1]." With the subject then under investigation, any letter (*e.g.* Y) immediately tended to call up a number (25) expressing its position in the alphabet. Such a tendency may readily yield the above diagram, where consecutive scales are associated with consecutive spectral colours, and may also result in the strong inclination towards mysticism which is characteristic of Scriabin. For him the (red) key of C relates to matter, and is redolent with the odour of the soil, whereas the (violet) key of F♯ is spiritual and ethereal. He believes that colours have their over-colours, as tones have their overtones.

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[1] *Loc. cit.* 238.

II.

The other case of synaesthesia here recorded is in a lady (Subject *B*) who is an accomplished painter and takes a keen enjoyment in hearing music although she does not play on any instrument.

Like the case (Subject *A*) described in my previous paper, this subject has very poor visual imagery, and the colours which she obtains vary with the pitch and with the timbre of the tone.

A visual image only comes to her when she has taken special care to attend to the previous perception of the object. Thus, when painting, she can visualise her 'sitter' if she has attended particularly to the pose. But she cannot get a visual image of her breakfast table, "because she has never attended to it." Nor does she 'see' colours in sounds; she explains her coloured hearing on the ground that sounds "give her the same mental sensations" as colours.

The flow of colours she experiences in listening to music afford her "enormous pleasure." They vary with the composer; the works of Chopin, for example, yield "very translucent colours such as green leaves in the spring," whereas those of Schumann "never give 'primary' colours, they give purples and the like,—not transparent colours." The colours come more reliably when they are not specially looked for: "it is so difficult to be truthful when one is watching."

Individual tones have each a colour dependent on their pitch; but the colours, as given in the following table, show a sequence very different from that described in the case of *A* in my previous paper.

Pitch of tone	Colour
256	Prussian blue, clear blue.
300	A clear mixed colour, a suggestion of streakiness, dark blue streaked with violet.
400	Clear dark violet—clear purple.
500	Deeper than red, very deep golden, transparent.
600	No definite colour, opaque, streaky, perhaps black and flame colour.
700	No definite colour, uninteresting, perhaps light green.
800	Blue.
900	Rather like 800.
1200	Might be yellow, something of that nature, very translucent.
2048	Getting yellow.

The above colours were obtained for the tones of tuning forks, sounded in irregular order. Several of the tones were repeated at the same and at a later sitting, and gave confirmatory results[1].

For higher tones, the whistles supplied with the Edelmann tone series were employed. When the whistle giving tones from about 1704 down to 1024 vibrations per second (a^3 to c^3) was sounded, *B* replied that the highest notes were a faint green, or yellow-green, while the lower became "more blue, less yellow." The tones of the next whistle, ascending from about 1704 to about 3408 vibrations per second (a^3 to c^4) were described as becoming "more and more emerald-green." The Galton whistle tone of 6000 vibrations per second (about g^5) appeared green, ascending from which still higher tones became increasingly colourless.

That is to say, in this subject we appear to have (i) a change in hue from blue through purple and reddish-yellow to green, for tones varying from about 200 (256) to 700, and (ii) a broadly similar repetition of this flow of colours—from blue through yellow to green—for tones ascending from 800 towards the upper audible limit. The blue of 200 became more and more purple as the lower limit was approached, *e.g.* "much darker and more purple than the gentian" (128), "still more purple" (64), etc.

It was remarked by the subject that the colours almost invariably appear to her as uniform or streaky (streaked with 'black' or 'light'), clear or opaque, smooth or rough. Yet there was "no form in the colours"; hence she could not explain, for example, in what direction the streaks were running.

As in the case of Subject *A*, described in my previous paper, traces of the colours obtained by two single tones were often also obtainable for two simultaneously sounding tones. Thus the simultaneous tones, 300, 600, were described as "something very dark,—either blue or black,—and something clear and light"; for the simultaneous tones 300, 500, she reported—"that has that horrid Prussian blue in it,—a colour I loathe."

The simultaneous tones, 600, 900, were described as "an acid mixed sound. To me there are three colours in it. For the moment I can't name them. One is black. They all seem very clear colours. (You can have a clear black as in looking into a pond at night.) There is a streakiness in the colours—a black and two things bright and crude."

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[1] The only exception was a failure at the second sitting to obtain gold or flame colours for the region 500—600. It appeared to behave like the region 700—800. Thus 500 suggested a streaky green, 512 an intense green, 600 a streaky black and clear light blue.

Like *A*, *B* is more 'alive' to the colour components than to the tonal components of sounds. She "may be aware of the presence of several colours where the number of simultaneous tones is not attended to."

Again as with *A*, the effect of increasing the richness of a tone by adding to its overtones results in a 'rise' of the colour of the tone.

Source of sound	$c^1 = 256$	$c^2 = 512$
Tuning fork (with resonator)	Clear blue	Rose colour in blue
Tonmesser	Reddish blue	Bright yellow
Tone variator	Indigo blue	Greenish
Horn	A horrid red colour	
Clarinet	A mixture of yellow and green	

THE METHOD OF COMPARISON[1] IN EXPERIMENTS WITH MUSICAL INTERVALS AND THE EFFECT OF PRACTICE ON THE APPRECIATION OF DISCORDS.

BY C. W. VALENTINE,

Lecturer on Psychology to the St Andrews Provincial Committee for the Training of Teachers.

I. *Purpose of experiments.*
II. *Method of experiment.*
III. *Individual variations in preferences for intervals.*
IV. *Comparison of results given by the two methods.*
V. *Variations of the aesthetic attitude according to the method used.*
VI. *Disappearance of dislike of discords with practice.*
VII. *Summary of results and conclusions.*

I. Purpose of the Experiments.

IN order to supplement the general results, gained from large numbers of subjects, in the experiments described in two previous papers[2], a more thorough investigation of five subjects was undertaken. My aim in these supplementary experiments was also to test the value of the method of comparison in aesthetic investigations such as the present.

Accordingly in one series of tests the subjects were asked to give judgments of preference upon pairs of successive intervals. They also submitted to another series of tests in which they gave an absolute judgment upon single intervals.

Very divergent opinions have been expressed upon the value of the method of comparison as applied to aesthetic judgments. G. Kaestner[3], who also used the method of comparison in experiments with musical intervals, strongly advocates the use of this method. He maintains, (i) that it avoids the difficulty of frequent "judgments of indifference" likely to be given with the serial method; (ii) that it is easier to compare two intervals than to express a judgment upon one; (iii) that momentary moods and feelings, dependent upon the process of perception itself, are more disturbing in the serial method.

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[1] The usual name for this method is the 'method of paired comparisons' which seems inexact.

[2] This *Journal*, 1913, VI. 190–216, and 1914, VII. 68–111.

[3] "Untersuchungen über den Gefühlseindruck unanalysierter Zweiklange," in *Psychol. Stud.* IV. 476.

Külpe, in his paper at the Würzburg Congress for Experimental Psychology[1], assumed a cautious attitude towards the method of comparison, remarking that a testing of the method as a method for experimental aesthetics was greatly needed. At his suggestion L. W. Legowski tried the method in experiments with lines and geometrical figures[2]. He concluded that it is markedly inferior to the serial method, and mentions particularly that the latter gave far better introspective results.

E. Bullough, criticizing the method of comparison, says, "to demand preference judgments seems to me to be fundamentally and radically opposed to the aesthetic attitude in general," "it is primarily characteristic of the aesthetic attitude to be non-comparative, individualising, isolated, and in a sense absolute[3]."

It seemed possible to test the value and the reliability of the method in two ways: (1) By finding to what extent the method of comparison gives the same results as the method of successive presentation, as regards the order of preferences for musical intervals; (2) by finding how far the aesthetic attitude, as revealed in the introspective remarks, is affected by the method of comparison.

II. Method of Experiment.

The plan followed was to carry out a series of experiments by the method of comparison, and at the same time, interspersed with these, a series of tests by the method of successive presentation of single intervals. Both methods were used at each sitting. A new Bechstein piano was used throughout the experiments.

At one sitting the twelve intervals within the octave would be played one at a time, the subject being asked to express his judgment upon each interval and to give the reason for it in the way described in my previous paper[1]. At the end of this series some dozen *pairs* of intervals were played, preference judgments being asked for. At the next sitting, a series of pairs would be given first, the series of intervals presented one at a time would come after, and so on. When the pairs were presented the subject was asked to say not only which he preferred but why he preferred it. Subjects were asked to judge each interval upon its own merits, and to avoid, as far as possible, being influenced by the interval with which it was compared. If such influence was detected they were asked to state its nature.

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[1] "Der gegenwärtige Stand der experimentellen Ästhetik," in *Ber. ü. d. II Kongress f. exp. Psychol.* 1906.

[2] "Beiträge zur experimentellen Ästhetik," *Arch. f. d. ges. Psychol.* XII. 274.

[3] "The Perceptive Problem in the Aesthetic Appreciation of Colours," This *Journal*, II. 412.

In the comparison method each interval was compared with each of the others, in haphazard order, care being taken that the same interval should not recur too frequently in any part of the series. When the whole series of sixty-five comparisons had been worked through thus, the series of pairs was repeated in the reverse order. The order of the two intervals of each pair was also reversed, the interval which had previously been played first of the pair now being played second of the pair. The notes were held down for three seconds, then followed a pause of three seconds, whereupon the second interval was played, this also being held down for three seconds. The times were regulated by a stop-watch kept going continuously. After another pause of about five seconds the intervals were played again in exactly the same way. This repetition of the pair, resulting in a series *A* ... *B* ... *A* ... *B*, probably equalised the effects of succession as far as is possible with this method, for *A* not only preceded *B* but followed it, and similarly with *B*.

In the series of successive presentations of single intervals, each interval was played for three seconds, and again after a pause of three seconds. All the twelve intervals were played at one sitting in haphazard order. At the next sitting this order was exactly reversed. At the next sitting a new order was used and so on, so that, in all, twelve different orders were used. When the writer acted as a subject, subject *A* acted as operator.

The subjects, who were all personally well known to me, may be described briefly as follows:

A (woman). Very fond of music, including classical music; a fair pianist; has had some practice in introspection in various psychological experiments.

B (woman). Very fond of music, but has no knowledge of the theory of music; no practice in experimental work in psychology.

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[1] This *Journal*, 1913, VI. 191.

C (man). Moderately fond of music of a popular rather than classical nature; has a slight knowledge of theory: a good introspectionist.

D (man). Fond of music; has some knowledge of theory; an expert introspectionist.

E (man). Very fond of music, including classical music; only very moderate pianist, but plays by ear with some facility; well practised in introspective work in experiments.

III. *Individual Variations in Preferences for Intervals.*

Before proceeding to the strictly methodological inquiry we will consider the marked individual differences which appear between the subjects. Each interval was judged thirty-four times by each subject, twelve times by itself (Series I), and twenty-two times in comparison with some other interval (Series II). Table I shows the results by both methods. In Series I the same system of scoring was adopted as that described in the previous experiments[1]. In Series II, an interval scored + 1 when it was preferred to the other with which it was compared, this latter scoring – 1. If both were judged equally pleasing, neither scored anything.

If we consider Table I we see, in the first place, that great differences appear in the orders of preferences of different individuals. Thus in Series I the octave is the most pleasing interval with *A* and *B*, but comes seventh and eighth respectively with *C* and *D*; the tritone, one of the three best liked intervals with subject *E*, is eighth with *B*; the fifth, which scores only one less than the highest score in the case of subject *E*, viz. $13\frac{1}{2}$, scores – 22 with *C*. With the discords there is greater similarity between the five subjects; but, even here, the major second scores + 9 with *E*, but – 23 with *C*, and the minor seventh varies between + 7 and – 26. Only the minor second is at or below the pleasure threshold for all subjects.

It will be seen that the order of degree of pleasingness of the more consonant intervals for any one individual may differ greatly from the average order based upon the judgments of all the five subjects, or upon the judgments of the one hundred and forty-six subjects who were tested in the previous investigation. Here then may be one very fundamental factor which may help to determine individual variations of taste and opinion in reference to musical compositions.

It will also be seen that in no case is the order of preference determined by the degree of consonance of the intervals.

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[1] This *Journal*, 1913, VI. 195.

TABLE I.

SERIES I. Method of Successive Presentation of Single Intervals.

Subjects ...	*A*	*B*	*C*	*D*	*E*	Totals	Average Order
Maj. third ...	12½	9	19	11½	7½	59½	1
Min. third ..	10	5	14	10	12½	51½	2
Octave	18	15	-16	6	13½	36½	4½
Maj. sixth ...	4	11	-10	10½	14½	30	7
Min. sixth ...	2	8	11	9	14½	44½	3
Fourth	0	5	6	10½	13	34½	6
Tritone	5½	2	7	6½	14½	35½	5
Fifth	-1½	11	-22	10½	13½	11½	8
Maj. second...	-9½	-1	-23	1	9	-23½	9
Min. seventh	-15	7	-26	-9	-1	-44	10
Maj. seventh	-22	6	-28	-13½	-6	-63½	12
Min. second	-17	0	-26	-11½	+1	-53½	11
Balance of votes	-13	+66	-94	+41	+106	—	—

SERIES II. Method of Comparison.

Subjects ...	*A*	*B*	*C*	*D*	*E*	Totals	Average Order
Maj. third ...	11	7	22	7	4	51	1
Min. third ...	5	-1	18	14	2	38	2
Octave	20	14	-10	-7	16	33	3
Maj. sixth ...	12	1	4	7	6	30	6
Min. sixth ...	3	0	12	7	9	31	4½
Fourth	4	6	8	1	3	22	7
Tritone	2	-2	5	18	8	31	4½
Fifth	6	8	-6	5	5	18	8
Maj. second...	-10	-14	-7	-1	-10	-42	9
Min. seventh	-16	0	-17	-10	-17	-60	11
Maj. seventh	-20	-4	-20	-20	-16	-80	12
Min. second	-16	-13	-9	-20	-1	-59	10
Consistency Score	$\frac{52\frac{1}{2}}{66}$	$\frac{47}{66}$	$\frac{51}{66}$	$\frac{45}{66}$	$\frac{43\frac{1}{2}}{66}$	—	—

Explanation of consistency score in Series II: As each pair of intervals was presented for comparison twice, the second time in the reverse order to the first, sixty-six comparisons were made in each half of Series II. The consistency score indicates the extent to which the subject expressed the same preferences in Part II as he had done in Part I of Series II, the maximum consistency score being 66.

Very great individual differences also appear as regards the balance of pleasing over displeasing judgments or *vice versa* in Series I.

The balance varies from + 106 (subject *E*) to − 94 (subject *C*). It is noteworthy that those subjects for whom the intervals are most nearly equal to one another as regards pleasingness are most pleased with them as a whole. This is brought out in Table II.

TABLE II.

Subject	Balance of votes on all intervals	Most pleasing interval receives	Least pleasing interval receives
E	+ 106	+ 13½	− 7
B	+ 66	+ 11	− 1
D	+ 41	+ 11½	− 13½
A	− 13	+ 18	− 22
C	− 94	+ 19	− 28

IV. Comparison of the Results given by the two Methods.

If the totals of all the subjects are added, the order of preferences for the twelve intervals is remarkably alike for the two series.

TABLE III. *Average Order of Preference for the Intervals.*

Method of Successive Presentation of Single Intervals	Method of Comparison
Major third	Major third
Minor third	Minor third
Minor sixth	Octave
Octave	Minor sixth
Tritone	Tritone
Fourth	Major sixth
Major sixth	Fourth
Fifth	Fifth
Major second	Major second
Minor seventh	Minor seventh
Minor second	Minor second
Major seventh	Major seventh

In view of the similarity between the two orders in Table III we may conclude that there is no musical factor constantly and singly at work in the comparison method, influencing the judgment upon certain intervals more than upon others, I mean a quality inherent in the musical nature of the interval itself, making it, for example, invariably appear to greater advantage when taken with another than when alone.

But when we consider individual results we find some marked differences produced by the different methods, differences which must be traced to different perceptive attitudes adopted in the two methods. Thus with subject *A*, the tritone drops from the fourth position in Series I to the eighth position in Series II, the fifth rising from eighth place to fourth. With subject *B*, the fourth rises from eighth place to fourth, the major seventh dropping three places. With subject *C* there is a difference of sixteen votes between the tritone and the major sixth in Series I, but the two are practically equal in Series II. With subject *D*, the tritone rises from seventh place in Series I to first in Series II, and the major third drops from first place to third, the minor third rising three places. With subject *E*, the octave, though practically the same as the fourth in Series I, is easily first in Series II, while the fourth drops to seventh place.

We must then conclude that the particular method followed in obtaining judgments upon musical intervals makes a great difference to the results obtained, even when every care is taken to balance as far as possible the contrast effects which are likely to be produced, especially by the method of comparison.

Furthermore we see that we cannot trust to introspections to detect the influence of one interval upon another, unless observers are specially trained. Subjects *B* and *C* thought that their judgment on each of the intervals in the pairs was quite independent of the influence of the other interval: and *A* thought such influence very slight and confined to the five cases out of 132 judgments when she mentioned it. Only *D* and *E*, who had had much more practice in introspection than the other three, found such influences constantly present, *E* especially feeling that they were unavoidable.

V. *Variations of the Aesthetic Attitude according to the Method used.*

Table IV shows the numbers and types of introspective remarks given during the two series of experiments.

TABLE IV. *Showing comparative frequency of different types of judgments according to the method used.*

	Subjects.........	*A*		*B*		*C*		*D*		*E*		Totals	
	Series	I	II	I	II	I	II	I	II	I	II	I	II
Types of Judgments	Character	20	10	7	4	2	1	0	2	21	21	50	38
	Associations (Musical)	0	0	0	0	0	0	6	0	7	0	13	0
	Objective	68	100	53	63	63	38	20	50	21	30	225	281
	Subjective......	18	7	4	2	6	0	2	3	11	5	41	17
	Non-musical Associations	4	1	0	0	0	0	1	2	3	0	8	3
	Totals	110	118	64	69	71	39	29	57	63	56	337	339

Series I = experiments with single intervals. Series II = experiments by method of comparison.

Legowski, in his experiments with figures, found that the comparison method proved much inferior as regards the material gathered from introspection. In our musical experiments the total *number* of introspective remarks in Series II is practically the same as in Series I. The comparison method is much less fruitful in the case of subject *C*. It is more fruitful for subject *D*, but this subject found the complex process of comparison *intellectually* very interesting. The great majority of his introspective remarks in Series II (37 out of 57) related to the influence of one interval upon another, and were not really judgments upon the intervals as such. This applied also to many of *E*'s judgments in Series II and to a number of *A*'s. If these were subtracted the method of comparison would give a considerably smaller number of genuinely aesthetic judgments upon the intervals than is given by the method in which single intervals were presented.

Furthermore we are concerned here with the *nature* of the judgments rather than their *number*, and it will be seen that the variety of judgments was greatly decreased by using the comparison method. The number of judgments of the 'objective' type was considerably increased but all the other types were decreased in numbers.

It is not surprising that the necessity of comparing the two intervals has a tendency to force one into an objective attitude. The sound is, after all, such a simple thing and its aesthetic impression so fleeting that it cannot, as a rule, hold the emotional mood it has roused when another interval is presented with its appropriate aesthetic impressions also struggling for existence. We either get an aesthetic impression due to the whole formed by both intervals (as was sometimes stated), and then there is no real comparison; or if this does not happen there is a tendency for a purely critical attitude to be adopted, and especially for some one standard to be selected by which the two shall be judged, *e.g.* the degree of consonance. Here we have the clue to some of the marked differences between Series I and Series II as regards the order of preferences for the twelve intervals. With subject *C* the minor sixth fared much better in the comparison method than in Series I. In Series I it was found "dismal," "melancholy": and was therefore disliked nine times out of twelve. When compared with others this aesthetic aspect drops into the background; and so the sixth was preferred to the discords because it is "less discordant," and to the octave because it is "not so wide."

With subject *A*, the tritone fared better in Series I. It was pleasing because "soothing," "gentle," "soft," "sad," "plaintive" (six times),—'character' judgments largely. On one occasion the struggle between the two attitudes 'character' and 'objective' is shown in the remark "Very hard to decide; sometimes I notice the discord, sometimes it appears as plaintive." Now the method of comparison at once turns the scale, and the objective attitude is predominant throughout: the interval appears "less clear," "less harmonious," or positively "discordant," the plaintiveness only being mentioned once.

In the case of subject *D*, the influence of the sequence or of contrast on the judgments upon the tritone (which differed so much in its order in the two series) was mentioned fourteen times in twenty-two judgments on that interval.

As to the general effect of the method of comparison, individual differences are again obvious. Subject *B*, a young German lady, seemed to be the least affected by the change of method. With her the two intervals of the pair seemed to hold their own independent aesthetic impressions well. Thus the two intervals were often judged to be equally pleasing (55 times out of 132), suggesting a frequent failure of real comparison. Indeed *B* often asserted that the two intervals could not be compared, the grounds of their pleasingness or displeasingness being so different. For example, rightly insisting on a difference between "pleasing" and "interesting," she sometimes said that one chord was more pleasing (*angenehm*) but the other more interesting (*interessant*)[1]. On these occasions a judgment of "equality" was given; that is, the intervals were said to be equally pleasing.

With *E* the 'character' aspect of the intervals was sufficiently strong to remain even in comparison. The objective judgments increase only at the expense of the subjective and associative. To *E* the comparison method appeared more satisfactory only in one way, namely that it completely kept out the association of the Dead March in *Saul* with the major third, which *E* could not get rid of in Series I, and which had resulted in the major third coming only ninth on the list. In Series II it was sixth on the list. Probably, after *B*, subject *E* was least affected by the change of method, holding the aesthetic impressions of the intervals more independently than did the others. This would explain too why he comes next to *B* in the number of judgments of equality (viz. 17) some at least of which were due to inability really to compare the intervals. *A* gave only five "equal" judgments, *C* and *D* none.

Both *D* and *E* were considerably influenced by the effect of expectation in the comparison method—the second interval often being unpleasant mainly because of surprise. On the other hand a very close resemblance of the second to the first often made it displeasing "through monotony."

In conclusion we may say that none of the advantages claimed for the method of comparison are supported by these experiments, particularly if we study individual cases and do not rely on a mere average of results. Kaestner's assertion that it avoids the difficulty of frequent judgments of indifference is counterbalanced by the fact that nearly half the one hundred and thirty-two judgments of *B* and seventeen of those of *E* were judgments of equality.

Doubt is thrown upon his statement that it is easier to compare two intervals than to express a judgment on one. One subject took refuge in judgments of equality: all observers, except *C*, often felt the comparisons to be very difficult and in the case of *D* the intellectual act of comparing frequently seemed to absorb the attention and to prevent a really aesthetic judgment upon the intervals. The interest *D* showed in the mere act of comparing accounts for the fact that he alone shows a large increase in the number of introspective remarks in the method, and suggests that "feelings dependent upon the process of perception itself" are much more likely to be disturbing in the method of comparison—just the opposite to what Kaestner asserts[1].

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[1] This distinction was made quite spontaneously, and the subject knew nothing of psychological or aesthetic discussions about hedonism.

If we subtract those introspective remarks which had reference merely to the *relation* of the intervals to one another or to the act of comparing them, the total amount of introspective material was less in the experiments by the comparison method. Also the variety of the judgments was much less, thus supporting the finding of Legowski that the method does not give such good introspective results.

Finally Bullough's criticism is supported by the frequent changes (mentioned above) from an aesthetic attitude, assumed when an interval was presented alone, to a more purely intellectual, critical way of judging, when the intervals had to be compared.

It is obvious, from our results, that individuals may differ enormously in the way that they are affected by the method of comparison. Subjects *C* and *E*, for example, said they were able to "hear the intervals over again in the mind," and *C* said he could do so with ease in the *reverse* order to that given in order to judge better; but many are doubtless unable to do this.

Further, as regards the general value of the method of comparison in experimental aesthetics, much depends on the objects that are being compared. Musical intervals are comparatively simple things, and the sounds were only of a few seconds' duration. With more complex objects such as pictures, for which the subject may be given his own time and which have sufficient individuality and power to make a very decided and permanent impression, the method may not suffer so much from some of the imperfections which have been found in these experiments. Yet against this is to be set the fact that the very strength and permanence of the impression due to picture I of a pair, determines all the more decidedly the attitude in which we shall receive picture II, which has to be compared with it; so much so that a picture *A* which, taken alone, is found displeasing, may be liked when seen after picture *B*, or, if found pleasing when seen alone, may be disliked when seen after picture *B*[1].

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[1] An interesting illusion occurred with *D*. He expressed the opinion that towards the end of the experiments he judged each chord more independently because the interval between the chords was longer. This idea that the intervals were longer was quite a mistake, as the interval remained the same (3 secs.) throughout. When *D* was informed of this he agreed that the illusion was probably due to the fact that he decided how he felt towards the first interval more easily and quickly as the experiments progressed and so the intervening time appeared less filled and therefore longer. This illusion is similar to that in which time intervals filled by activities of the subject, such as reading or counting, appear shorter than empty intervals of the same length. In the case of short intervals filled by sensory impressions made upon the subject the filled intervals appear longer. See C. S. Myers, *Text-Book of Experimental Psychology*, 2nd edit., p. 297.

VI. *Disappearance of Dislike of Discords with Practice.*

The five subjects in the present investigation expressed their judgment upon each of the intervals thirty-three times in the course of one or two weeks. It was found that in the case of two of these subjects (*B* and *E*) the discords became much less displeasing as the experiments proceeded, rising indeed almost to the hedonic level of the concords (see graphs below). In the case of a third subject (*D*), only one of the discords became pleasing instead of displeasing in the course of the experiments, the successive judgments (in Series I) upon the major second being as follows: very displeasing, very displeasing, slightly displeasing, pleasing, slightly pleasing, pleasing, pleasing, pleasing, displeasing, pleasing, pleasing[2].

With subject *B*, all four discords change from displeasing or indifferent to indifferent or pleasing towards the end of the experiments, the balance of all scores of the four discords being -1 for the first four presentations of each, and $+10$ for the last four.

With subject *E*, three of the discords change from displeasing to pleasing. The other discord (major second) was found pleasing from the beginning. The total scores of these three discords change from $-8\frac{1}{2}$ for the first four presentations to $+7\frac{1}{2}$ for the last four.

The remaining two of the five subjects show practically no change of this kind. These subjects, *A* and *C*, were the two who were by far the most critical towards the intervals (see Table I).

There is no general increase in the pleasingness of all the intervals to account for the changes in reference to the discords. If this had been the case we might have supposed that the whole attitude of subjects *B* and *E* changed towards the intervals in general in the course of the experiments. But the pleasantness of the concords remained fairly constant throughout the experiments, as is shown by the graphs below. In these graphs the plain lines indicate the averages of the judgments of the subject at each sitting on the eight most consonant intervals within the octave, and the dotted lines indicate the averages of the judgments at each sitting on the four dissonant intervals.

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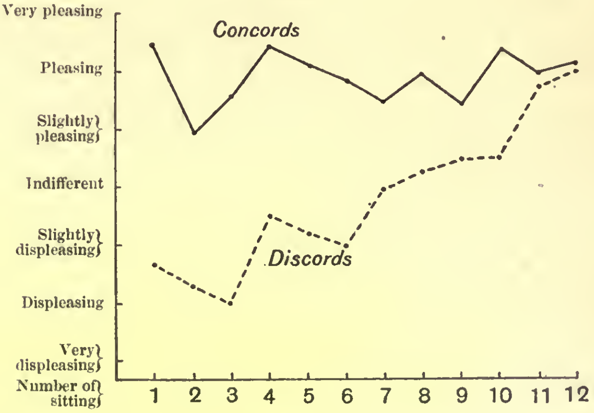
[1] See the writer's *Experimental Psychology of Beauty*, pp. 90–92.

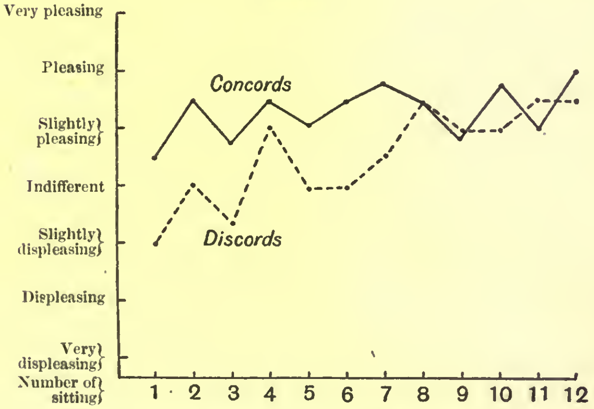
[2] These are the judgments given in the series of experiments by the method of presentation of single intervals. As a series of experiments by the method of comparisons was also taking place as a parallel series to the above the subject would have pronounced judgment upon the interval sixteen times by the time the middle of the series given above was reached.

Graphs indicating change in the appreciation of discords with practice.

Subject *E.*



Subject *B.*



So far we have referred only to the changes in the judgments upon the discords which took place in the course of the experiments with single intervals. In the case of subject *B* a similar change was also remarkably well shown in the experiments by the method of comparison. The total score of the four discords in the first half of these experiments was – 21, in the second half it rose to – 10, the total score of the eight concords dropping from + 22 to + 11. The change is shown only to a slight extent by *E* in the experiments by the method of comparison. This may be partly due to the nature of the method forcing the subject into the objective attitude and especially bringing forward the degree of consonance as the chief criterion for comparison. But even during the comparison experiments there was some evidence of change as regards the appreciation of the discords, in the increasing hesitation shown, not only by subject *E* but also by subject *A* in judging the discords less pleasing than the concords. It will be obvious that the actual scores of the discords in the comparison method would indicate the change of attitude towards the discords only if the discords became as pleasing as, or more pleasing than, some of the concords. The scores would fail to indicate any improvement on the part of the discord which fell short of this. This in itself is a further disadvantage of the comparison method. From the graph of subject *B* it will be seen that in her case in two sittings near the end of the experiments the discords were actually judged to be more pleasing, on the average, than the concords.

We turn to the introspective remarks for a clue to the nature or cause of these changes of the appreciation of discords. In the case of subject *B*, discords at first spoken of as "not clear," "harsh," or "displeasing to the ear," were called towards the end of the experiments "interesting," "pleasing," and even "sweet." Subject *E* at first wrote thus of three of the discords: "unpleasant sensation in the head," "unable to apprehend the notes as one; they fight," "the start jars." At the end of the experiments he said of the same discords, "interesting, seems to involve mental activity," "plaintive," and "stimulating."

These introspective comments indicate to some extent the nature of the change in the appreciation of discords. Taking them in conjunction with other evidence, we have in this phenomenon probably something more than a mere change of attitude towards an unchanging presentation, something which may perhaps aptly be called accommodation or adaptation to discords. Both in these experiments and in those described in my previous paper there was evidence that dissonant intervals when pleasing may actually be 'felt' as consonant.

Thus a subject sometimes ventured a surmise that a given interval was a certain concord, when really it was a discord, and sometimes the subject had the idea that all the discords had not been played in Series I at a given sitting. Sometimes again the discordance of an interval actually seemed to disappear on its being replayed. It would seem possible, then, that this lessening with practice of the unpleasantness of discords, and the development of a condition in which they are found pleasing, may indicate that even in a single series of experiments, if sufficiently long, some adaptation to discords may take place comparable to what seems to have taken place within the history of European music.

The latter change is of a permanent nature, so far as we can see. Possibly it is largely determined by the accustoming of children to the music of their generation at the period of special plasticity for such adaptation, whatever that may be. Unfortunately I have not been able to retest my subjects to see how far the adaptation to discords was permanent[1]. The subjects were scattered before I discovered this adaptation, otherwise I should also like to have found whether there was any correlation between adaptability to discords and a liking for the works of some composers of the modern French school. I can only say that one of the two subjects who showed a marked adaptation to discords (subject *E*) greatly enjoys 'modernist' compositions, while Wagner was the favourite composer of subject *B*, the only other subject who showed such adaptation.

The phenomenon of adaptation discovered in these experiments may also be similar to that which Max Meyer found in his experiments with the quarter-tone music composed after the style of some Asiatic music[2]. Most of his subjects found the music very displeasing at first, but pleasing after a dozen or more repetitions. He himself found that the music at first was highly disagreeable but that later it became more and more beautiful. Meyer says that some of his subjects had already acquired such a "definite set" in their way of thinking musically that they were very slow to adapt themselves to a style of music so novel. My third subject (*C*) may be of this nature. But he was by no means the one who was the most familiar with music—capacity for adaptation does not seem to depend on lack of previous musical training.

In the present investigation the second playing of a discord, after the interval of three seconds, was sometimes found less displeasing than the first, apparently because of a mental preparation or accommodation by which any shock or disappointment was lessened.

There seems to be also a similar type of adaptation when the discord is unpleasant at the moment at which it is struck and then becomes pleasing before the sound ends[1]. The writer noticed this repeatedly in his own case, and others testified to the same experience. Possibly the stage at which the discords became more permanently pleasing was only a continuation of this species of accommodation. In that case we may suppose that, with practice and complete familiarity, the accommodation takes place so rapidly that there is not time for any appreciably unpleasant feeling to arise, even if the interval were originally displeasing; or that the accommodation remains as a permanent modification, so that it is simultaneous with the very act of perception, just as experience generally determines that we shall perceive a thing as some definite object simultaneously with the sensation of colour and form, however vague and blurred these may be.

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[1] I did not think that a re-testing of myself would be of much value, owing to the possible influence of suggestion.

[2] "Experimental Studies in the Psychology of Music," *Amer. J. of Psychol.* XIV. 207.

Such accommodation would probably have as its psychological correlate not any mere adjustment of the end-organs concerned, but rather some modification of a higher brain process. It may thus be analogous to such accommodation as that of which Stumpf speaks when he says that we may apprehend a "neutral third" at one time as a major and under other conditions as a minor third[2]. And the kind of accommodation which we have found in these experiments, and possibly also that referred to by Stumpf, are probably of considerable importance in that general readjustment of our mental attitude which Myers has emphasized as so necessary if we are to appreciate and understand much of the music of primitive peoples[3].

If such striking changes can take place in our appreciation of discords in the course of a series of experiments, who can say what combinations of notes may not be enjoyed in the music of the future, as the successive 'modernists' of each generation accustom their hearers to newer harmonies?

The history of European music shows that progress has taken place in the direction of the admission and use of intervals previously forbidden. On the other hand it is possible that we have nearly reached the limit of progress in that direction, a limit set perhaps by the nature of the end-organs concerned, even if the higher processes are capable of further development.

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[1] Cf. the accommodation as to pitch of a neighbouring tone when another tone predominates. Stumpf, *Tonpsychologie*, II. 399. But the above mentioned accommodation is something more than that spoken of by Stumpf. It occurs with sevenths as well as seconds.

[2] "Tonsystem und Musik der Siamesen," in *Beitr. zur Akustik und Musikwissenschaft*, 3te Heft, p. 101. Cf. also the continued apprehension of the third or fourth as such, even where considerably mistuned, Stumpf und Hornbostel, "Die Bedeutung ethnologischer Untersuchungen für die Psychologie," *Ber. ü. d. IV Kongress f. exp. Psychol.* 1910.

[3] "The Ethnological Study of Music" in *Anthropological Essays presented to Edward Burnett Taylor*, Oxford, 1907.

This phenomenon of accommodation with practice, and the very partial interpretations given, are in full accord with, and, I think, lend support to a highly important suggestion as to the origin of our feelings of consonance, made by Myers. "Throughout our life, no tones are more closely associated with any given musical tone n, than one or more of its accompanying overtones $2n$, $3n$, $4n$, $5n$, $6n$, unanalysed though they be. And these are the very tones that form with one another the most consonant intervals. Thus harmony is seen to have a natural basis of association[1]."

VII. Summary of Results and Conclusions.

1. Very great differences appear between individuals as regards the musical intervals which they prefer, and also as regards the extent to which they find pleasure in the hearing of single intervals played on the piano.

2. The method of comparison does not give the same results as the method of the successive presentation of single intervals.

3. The effect of comparison is sometimes to change entirely the feeling of a subject for a particular interval. Unpractised observers cannot be trusted to detect the effects due to the use of the method of comparison.

4. The method of comparison was not found to be more fruitful as regards introspective remarks, as has previously been asserted. In particular the variety of aesthetic judgments was greatly decreased in experiments by the method of comparison and it tended to force upon the subject some one criterion by which the two intervals might be compared. When this tendency was resisted real comparison was often found impossible.

5. Objective judgments were more frequent with the method of comparison, character, association (both musical and non-musical) and subjective judgments being less frequent. With some subjects the interest in the intellectual work of comparison was apt to hinder the assumption of a truly aesthetic attitude.

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[1] *Text-Book of Experimental Psychology*, 2nd edition, p. 55.

6. Three of the five subjects in the course of the experiments showed a striking change in their appreciation of the discords, some or all of which became pleasing instead of displeasing. A change was sometimes noticeable even when the same discord was played again after an interval of a few seconds. Such changes suggest a kind of adaptation to discords, comparable perhaps to what has taken place in the course of the development of music.

7. This phenomenon of adaptation with practice to discords lends support to the theory that the perception of consonance and dissonance is dependent, partly at least, upon frequent association.

My thanks are due to those who so kindly acted as subjects in these experiments, viz. Fräulein Erhardt, Professor A. E. Taylor, Mr Basil Long, and my wife.

(*Manuscript received* 20 *September* 1912.)

PUBLICATIONS RECENTLY RECEIVED.

Ueber die Gesetze des geordneten Denkverlaufs: eine experimentelle Untersuchung. Von Dr Otto Selz. Erster Teil. Stuttgart: W. Spemann. 1913. S. viii + 320. M. 9.

The author of this work starts by calling attention to the inadequacy of the ordinary laws of association, constellation theory, etc., to account for the characters of orderly thinking. He sets out to ascertain the laws which regulate the control of intellectual processes by the 'determining tendencies' recognised by Ach, and the laws which regulate the co-operation of 'determining' and 'reproductive' tendencies. His experiments were conducted on principles similar to those adopted by Watt, Messer, and others who have worked on constrained associations: the subjects received definite instructions as to how they were to act before the stimulus-words were presented to them. But in these experiments the instruction was continually changing and was given simultaneously with the stimulus-word. The outcome of the author's experiments is to show the enormous importance of 'direction' in thinking, and leads him to replace the 'constellation theory' of reproductive processes in orderly thinking by what he calls a 'complex theory,' the essential of which is that an associated whole depends not on the constellation-units of which it is composed but on its own undivided 'complex' activity.

Il Metodo degli Equivalenti. Da Dott. Agostino Gemelli. Firenze: Libreria Editrice Fiorentina. 1914. Pp. 344.

The object of this work and of the experiments described therein (carried out in the laboratories of Kiesow and Külpe) is to show that the 'method of equivalents' furnishes a valuable instrument for psychological experiment. "I have set out," writes the author, "to inquire into the method of equivalents as used in the field of tactual sensations, so as to see whether any results can be obtained from it or whether it should definitely and justly be condemned to that oblivion to which the now already old researches of Camerer consigned it" (p. 13). The author's experiments were planned to determine the equivalence relation between tactual distances (*a*) under varying external conditions (*e.g.* different cutaneous regions, different constant distances, different tactual pressures), and (*b*) under varying internal conditions (*e.g.* differences in mental concentration, fatigue, visual and other sense-distance associations). The author believes that his experiments show "that the method of equivalents places the experimenter in a specially advantageous position. It is able to place the subject in an ideal position for harmonizing the results of psycho-physical experiments with those obtained by systematic introspection, carried on simultaneously or in parallel" (pp. 201, 202). The concluding chapters are devoted to a detailed analysis of the process of comparison, including the preparation and *Einstellung* of the subject, the importance of the *Hauptdistanz* and of the pause between constant and variable distances, the formulation of the judgment, etc.

Die Gestaltwahrnehmungen: Experimentelle Untersuchungen zur psychologischen und ästhetischen Analyse der Raum- und Zeitanschauung. Von Dr Karl Bühler. Erster Band. Stuttgart: W. Spemann. 1913. S. viii + 297. M. 7.50; geb. M. 8.50.

This book gives an account of experiments (i) on the thresholds for awareness of straightness and curvature of seen lines, (ii) on the estimation of the size of figures, and (iii) on the comparison, by proportion, of optical figures and of time intervals. The central point of his inquiry is the perception of proportion, and he deals in detail with the dependence of the 'proportion threshold' on the proportion ratio, on the size of the intervals employed, etc. The last chapter discusses the theory of the perception of proportion, of which he believes Weber's law to be a special case, the law thus receiving a new interpretation. He ascribes our comparison of proportions to a physiological mechanism which, he believes, enables us immediately to perceive whether a magnitude a is or is not related to a magnitude b as c is to d. He suggests that this apparatus works in much the same mechanical way as that of any other instrument of proportion, *e.g.* a balance with unequal arms or a Wheatstone's bridge, and that to the laws of such instruments those of its own sensibility conform.

Industries of the Feeble-minded. By A. BICKMORE. London: Bartholomew Press. 1913. Pp. viii + 66. 2*s.*

The author of this little work has been for ten years Craftsmaster at the Darenth Industrial Colony, of the growth of which he gives a short account. He describes the trades taught there and gives interesting information which cannot fail to be of help to those engaged in training the feeble-minded.

Psycho-Therapy: its doctrine and practice. By Dr ELIZABETH SEVERN. London: Rider & Son, Ltd. 1913. Pp. 211. 3*s.* 6*d.* net.

The Socialized Conscience. By Professor J. H. COFFIN. Baltimore: Warwick & York. 1913. Pp. viii+247. $1.25.

Mental Diseases: a text book of psychiatry for medical students and practitioners. By Dr R. H. COLE. London: University of London Press. 1913. Pp. viii+343. 10*s.* 6*d.*

The Purpose of Education: an examination of the education problem in the light of recent psychological research. By ST GEORGE LANE FOX PITT. Cambridge: University Press. 1913. Pp. ix+83. 2*s.* 6*d.* net.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY.

March 10, 1914[1].	Definition of the Sexual Instinct and its Relation to other Tendencies, by W. McDougall.
March 24, 1914[1].	The Biological Significance of Delusions, by H. Devine.
	The Importance of Disturbances of the Personality in Mental Disorders (*Discussion*), by Dr R. C. Rows.
May 2, 1914.	The Psychology of Play with special reference to the Value of Group Games in Education, by M. Jane Reaney (introduced by Dr Brown).
	An Attempt at an Exact Estimation of Character (*Discussion*), by E. Webb (introduced by Professor Spearman).

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[1] In conjunction with the Psychiatrical Section of the Royal Society of Medicine.

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VOLUME VII SEPTEMBER, 1914 PART 2

THE RÔLE OF REPRESSION IN FORGETTING (I)[1].

BY T. H. PEAR.

IN a psychological examination of the phenomenon of forgetting, one finds much justification for the view that there seem to be two kinds of forgetting. The difference between them, recognisable without much difficulty, is obscured, however, by the fact that in ordinary speech the same name—'forgetting'—is applied to both processes. The distinction between them may be illustrated by the following examples:

(1) I learn to-day a series of nonsense syllables of the ordinary kind. In a year's time, say, I may be able to remember only two of them. The others have apparently been 'forgotten.' This forgetting seems probably to be attributable to a simple 'decay' of memory, perhaps to be accounted for in the way in which popular psychology would explain the fact, viz. as a result of the limited retentivity of the brain. By no method of analysis can I discover any special reason why the syllables were forgotten, and I can often find reasons why the particular syllables which were remembered have been retained by the mind; reasons concerned with their frequency of repetition, vividness in consciousness, recency of appearance or position in the series learnt. These factors are, of course, those familiar to the investigator of the 'economy of memory.'

(2) I forget to post a letter, or I forget the name of a person. This case is obviously different from the one just quoted, for the interesting characteristic of the second example is that in many such 'slips of memory' the forgetting is only temporary: there often comes, soon after the forgetting, a vivid remembrance of the fact which has eluded the memory. Further, it is often the case that the forgetting takes place at the very time at which the person appears to desire most earnestly to remember the fact, and the remembering may occur at a time when he does not wish to remember the thing previously forgotten.

In this example we have, from the very facts of the case, no mere

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[1] A contribution to the Symposium presented at the Joint Meeting of the British Psychological Society, the Aristotelian Society, and the *Mind* Association, in Durham, July, 1914.

'decay' of memory. Rather must we suppose that at the time there are present in the mind forces which, for some reason or other, prevent recollection. It is easy to see that in the first example one process—retention—is concerned, and in the second case quite another process, viz. recall. Forgetting may conceivably be due to a deficiency in either of these factors.

It seems, then, reasonable to suppose that there are two kinds of forgetting. The first kind (perhaps we may speak of it as 'decay' for senseless material) may be conceived provisionally as conditioned almost or quite exclusively by physiological factors. The sole, or almost the sole, power at work in this case may be the capacity of the brain to retain impressions, and Ebbinghaus's 'curve of forgetting' may be a 'decay curve,' comparable, perhaps, in its meaning to the curves met with in physical investigations. Certainly there seldom appears to be anything capricious or particularly difficult to explain in the forgetting of the subjects who participate in the usual experiments on the economy of memory for senseless material.

But the forgetting in the second case is, apparently, of quite a different order. In the first place, the fact should be considered that those particular things which any one person forgets seem to be determined by his own 'mental make-up,' quite apart from his ordinary capacity to retain or recall. *A* and *B* may have equal powers of recall for nonsense syllables, as determined by experiment, yet *A* may be quite contemptuously astonished at the ordinary matters which *B* forgets, while *B* is likely to assume a similar attitude towards *A*. Secondly, there is a great deal of obvious truth in the observation that the things one forgets are not always determined by chance. The commissions he is asked to do for other people are more likely to be forgotten, by the average man, than transactions of the same importance which refer to his own comfort and well-being. These 'slips of memory' cannot always be ascribed to a cynical disregard of other people, neither can they be put down entirely to an attempt to save oneself trouble. The unposted letter or the unperformed errand may be the cause of great personal inconvenience to us at a later moment, and we often know this fact quite well before the 'slip' occurs. There is no mere 'decay' here: the remembrance of the unfulfilled promise usually appears eventually and with great vividness.

Again, in considering this subject, one must take into account the significance of two social conventions which appear to be almost universal among modern civilised people. I refer first to the expectation and unquestioned assumption that a healthy person will 'try to forget' a poignant grief or disappointment if it is irreparable. Now the various ways in which persons of different types attempt this task of forgetting are significant. The weakling may take to drink; a slightly stronger type may seek to encounter great danger or excitement; yet another may plunge himself deeply into the difficulties and stimulations of his work. But in every case, except that of the unfortunate who broods over the sorrow, there are undoubted signs of an attempt to achieve the same aim—the exclusion from consciousness of the painful memory.

Of this universally-acknowledged fact two different views may be taken. One is that such a temporary 'forgetting,' which often ensues as a result of volitional behaviour of this kind, must be a manifestation of psychical control, subconscious or unconscious, a process of active *repression*. The other view would be that it is unnecessary here to assume anything other or more than the 'side-tracking' which any mental process necessarily must suffer when other, more intense, processes enter into competition with it. Owing to this competition the 'side-tracked' memory fails to affect the mind with an intensity sufficient either to entitle it to a place in the focus of consciousness or to insure for it a subsequent greater degree of retention than that which it possessed when it was recalled into consciousness. But it seems undeniable that a memory which at any time has been so importunate as to necessitate the taking of active measures to 'swamp' its action cannot be said to have failed to reach a degree of intensity sufficient (other things being equal) to bring about its retention in the mind. Under ordinary circumstances we remember with the greatest ease many events which assuredly were never so intense in consciousness that they had to be expelled. The very fact that we have tried to drive a memory out of the mind may be taken as sufficient warrant that it has reached a high degree of intensity. Therefore the subsequent forgetting of the memory must be accounted for in some way which does not necessitate the assumption that there was insufficient vividness for the impression of the experience on the mind. On the whole, there seems to be good evidence for the existence, in mental affairs, of an active force of *repression*.

What processes make up this repression? They are claimed to be processes opposed to those which normally would bring the repressed fact into consciousness. These latter processes are usually taken to be perseveration and association, or if the view of some recent psychologists be taken, association only.

With regard then, to the latter. It is already generally acknowledged that the association of ideas may be facilitated by creating a 'constellation,' which, in course of time, gives rise to images, ideas or thoughts favourable to the emergence of the desired fact. When, for example, we experience difficulty in recollecting a person's name, we naturally try to bring into consciousness all those facts within our knowledge which are connected with the owner of that name. We may endeavour to image his appearance, his house, his last conversation with us or the contents of the last letter he wrote. All this is so well known to everyone that it scarcely merits consideration here. But if we admit the existence of such facilitation in the associative sphere, we may not be allowed to shut our eyes to the probable existence of the correlated antagonistic process, inhibition. When we adduce, as one of the factors in the attention process, the facilitation of 'relevant' processes, we acknowledge at the same time the great importance of a simultaneous inhibition of 'irrelevant' processes: we express this by saying that we 'shut our eyes' or 'shut our ears' to the calls of experiences which are not concerned with the one which, for the time, stands out in consciousness with maximal clearness.

One seems compelled, then, to regard facilitation and inhibition as different expressions of one and the same process: any process which is facilitated will hold the central point of consciousness by virtue of the very fact that the other competitors are held back. Probably this is because there is competition for the available mental energy, which is limited in amount. Again, if, when we 'prepare our minds' for a given subject, the explanation of the phenomenon lies in the 'sub-arousal' of associative paths connected with relevant matters, may there not be an opposite process, which we might describe as 'shutting our minds' to a subject, the nervous correlate of this being the 'sub-inhibition' of paths concerned with ideas associated with this subject?

If, then, the existence of a temporary inhibition, occurring alongside the ordinary processes of facilitation, be granted, it seems reasonable to suppose the possibility of a semi-permanent inhibition. Such a case is stated to occur when a memory is repressed, in order to avoid the 'unpleasure' which would be caused in the mind by its revival. Many such cases have been given in psychoanalytic literature. I will discuss in this connexion one of my own experiences.

In the analysis of a dream, reported by the writer in this *Journal*[1], a striking example of forgetting the death of a close friend was encountered. Not only was the forgetting disclosed, to the writer's utter astonishment, by a symptomatic action (the writing of a postcard to the friend, some months after his death), but the dream there analysed seems (unless we regard its contents as mere nonsense, an attitude which the orderly structure of the dream does not justify) to be capable of only one interpretation, that the friend returns to his former post. The reasons for the repression of such an intimate memory as the above have been given in the article quoted. However, the matter does not end here. Last November, two years and four months after the death, two years and two months after the symptomatic action, and one year and four months after the dream, I wished to send Christmas greetings to some friends who are living abroad. To my surprise, I could not recollect their name. Immediately I went over in my mind all the circumstances which connected me with them. The town in which they are now living (a town in which I know no other people) and their occupation in it came at once into consciousness, and it was quite easy to pass in review the names of the comparatively small circle of our mutual friends. For some minutes, however, the desired name obstinately refused to appear. At last there came the name of this friend who is dead, and about whom I had dreamt the dream referred to. The next moment I was conscious of the name I was seeking, and I saw for the first time the great similarity between the two names. Both begin with the same letter, and, while both names have two syllables, all the syllables in the two names contain the same vowel-sound. Thus, in a loose sense, they rhyme with each other; they are associated through similarity in sound. Further, the nine letters of the longer name contain, in almost the same order, the whole six letters of the shorter. There is thus ample cause for the superficial, external association of the names. But, over and above this, the two names are connected by the strongest of internal associative links. These friends were introduced to me by the friend who is represented in the dream: we often met in his company, and he often spoke of them to me. Most important of all, however, is the fact that it was to them that I wrote immediately on hearing the news of his death.

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[1] 1914, vi. 288—292.

For these reasons the forgetting of this name seems to me to be beyond all possibility of explanation by 'chance' or 'inattention.' I forgot no other names in compiling the list, a list which contained the names of people whom I knew much less intimately than those whose name was forgotten[1]. The intimate connexion of this forgetting with both the symptomatic action and the dream makes it almost impossible to avoid the conclusion that such a 'slip of memory' is determined by unconscious or subconscious factors.

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[1] I should mention that my memory for names is in no way unusually weak.

The other side of the question raised on p. 140 might also be mentioned here, viz. that society is unanimous in expecting that certain instructions and duties will *not* be forgotten. Punishment for forgetting such duties is usually considered to be just. Freud points out that, both in military service and in the service of ladies, failure to remember is considered to be synonymous with a not wanting to remember[1]. This again points to an almost general belief that forgetting is not entirely free from psychical control.

In this connexion a phenomenon of special interest is the forgetting of dreams. To most people it is a familiar fact that a dream which, on awaking, may be as vivid as, or more vivid than any succession of images or memories in their waking life, frequently differs from these latter in one striking respect, that a very short time afterwards it is totally forgotten. Such a characteristic of dreams is regarded by Morton Prince as forming "only a particular example of amnesia for dissociated states, such as abstraction, pre-sleeping states, hysterical crises, trances, psycholeptic attacks, hypnosis, suggested post-hypnotic phenomena, etc.[2]" But it has been pointed out, in reference to this view, that even after a dream has been *recorded* it is frequently completely forgotten. Now the recording was not carried out in a dreaming but in a waking state. We might not therefore expect the recorded dream to be subject to any influences other than those which are operative in the case of ordinary material remembered during the waking life. But that this seems not to be the case has been made very clear to me during the time in which I have recorded my own dreams. I have often found that on awaking at the usual time in the morning I could remember that I awoke in the night and wrote down a dream. Frequently, however, I could not remember any details, or even the theme of it. Let us suppose that I had awakened in the night, read through a short story, and at once recorded what I could remember of it. It seems scarcely likely that I should have completely forgotten it in this way. Further, I have been accustomed to subject my recorded dreams to subsequent early analysis, and, for the purposes of later reference, following Freud's procedure, have given them titles which indicate their themes. When, some few months later, I have met with a passage in my notes referring to one of these dreams by its descriptive title I have often been unable to remember a single detail of it, although not long before I had spent some considerable time upon its record and analysis. An example of this might be mentioned here. In waking life my auditory imagery is very scanty and far from clear. I am therefore interested in the fact that in dreams it is much clearer and more vivid, and plays a much more important part in my consciousness than when I am awake. One dream, which I recorded in all its remembered details, but could not analyse completely or account for all its factors, begins with a scene in which I hear an orchestra playing the *pianissimo* opening of Sibelius's "*Valse Triste.*" This is a feat of imagery of which my waking life seems to be entirely incapable, and the dream was, on this account, highly interesting to me. A few months after recording the dream, and unsuccessfully attempting to account for or to interpret it, I read in my notes a reference to the "*Valse Triste*" dream, and could remember nothing of it, except that a dream with such a title appeared in my collection. I even forgot the opening scene described above. It seems very unlikely that I should have forgotten so completely a story of similar length if it had interested me as much as this dream undoubtedly did. It is, I think, important to note the fact that this was a dream which had *resisted* analysis: the latent thoughts of which therefore were still unconscious. That the thoughts which give rise to a person's dreams are frequently of a nature unacceptable to him in waking life is, I think, a fact beyond dispute. It seems therefore not unreasonable to connect the special forgetting of some dreams with the unacceptable nature of the thoughts which probably lie behind them.

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[1] Freud, *Zur Psychopathologie des Alltagslebens*, Dritte Auflage, Berlin, 1910, p. 83. Jones, "The Psychopathology of Everyday Life," *Amer. J. of Psychol.*, 1911, p. 491.

[2] "The Mechanism and Interpretation of Dreams," *J. of Abnormal Psychol.*, 1910.

In studying Freud's general theory of forgetting one must acknowledge that the examples quoted in illustration produce very different degrees of satisfaction in one's mind. Some of the incidents appear to be quite satisfactorily explained, but in others the factors are traced in a far less convincing way. It must be granted, however, that associations which sound 'far-fetched' to the reader may have actually existed in the mind of the person in question. Some minds seem especially given to playing with superficial associations by similarity, of which the pun (a form of association indulged in rarely by some people, frequently by others) is only one example. This necessarily subjective side of such work will always remain a perplexing factor, which an increasing knowledge of the range of individual mental differences will only partially remove.

There are probably, however, many everyday cases of forgetting, 'slips,' bunglings and similar performances which are not caused by such deep-lying, intimate factors as Freud has unearthed. When I left an umbrella the other night at a lecture in a distant town I did not, I think, thereby intimate my reluctance to leave my audience. The hour was late, the night wet, I was tired and anxious to begin my long journey home. I can find for this event no deeper or more personal 'explanation' than the one which would usually be offered, namely, that I was 'inattentive.' But it is quite clear that my inattention was merely a contributory factor favourable to my forgetting, and that the mere suggestion of inattention does not explain why I forgot, at that time, just the umbrella and nothing else. Similarly, as Freud has pointed out, vague references to physiological states of the brain (about which we know at present almost nothing) are not *explanations* of forgetting. These brain-states may facilitate forgetting just as the darkness and loneliness of a street may facilitate the losing of one's watch to a robber[1], but in neither case is the loss to be attributed entirely to these favourable conditions. It is possible that, just as darkness and loneliness are not essential factors in losing one's property, the presence of such brain-states as fatigue and similar conditions may not be essential to forgetting. In any case, until the great riddle of the relation of body to mind is solved, we are probably justified in seeking for the mental phenomenon of forgetting, a mental cause.

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[1] Freud's example, *op. cit.* 22.

THE RÔLE OF REPRESSION IN FORGETTING (II)[1].

BY A. WOLF.

MR PEAR'S attitude towards the Freudian view of the rôle of repression in forgetting is so cautious and guarded that I find it somewhat difficult to indicate briefly points of agreement and disagreement. Moreover, Mr Pear begins by emphasising a distinction which does not appear to me to be material to the problem to be discussed—I mean the distinction between retention and recollection. On the other hand, he overlooks the distinction between 'repression' and 'resistance,' a distinction which appears to me to be of first rate importance for the purpose of this discussion. Perhaps the best course for me to pursue will be to state my view of the question first, and to let the divergences between Mr Pear's and my own views come out by the way.

The suggestion that the forgetting of certain psychical contents is due to our repression of them has been brought into prominence by Freud and his followers, primarily as affording a convenient way of colligating and describing the facts relating to certain cases of hysteria and kindred phenomena, and as offering some kind of tentative explanation of their successful treatment by means of psychoanalysis. Taking his cue chiefly from the resistance with which he met when trying to elicit the past history of his patients, Freud was led to the supposition that there are certain forces subconsciously at work in their mind which prevent the emergence into clear consciousness of the forgotten experiences in question. And he conceived the general course or history of hysterical and similar neurotic cases to be somewhat after the manner of the following outline. Some idea or wish is aroused which is incompatible with the other ideas, beliefs, or tastes of the particular individual. Hence an inner conflict, which is ended by the repression of the irreconcilable wish or idea. The repressed idea is then forgotten. But it is not really suppressed. It survives in the unconscious region of the mind, and would emerge again into clear consciousness, were it not for the continued resistance offered by those hostile ideas which had originally repressed it. Still, although the repressed idea or wish does not emerge into consciousness, it nevertheless creates trouble by attaching its own painful sensations to all sorts of disguised emissaries or substitutes which can enter consciousness without encountering resistance. The symptoms of the hysterical and other neurotics generally consist of such surrogate bearers of the painful sensations of the originally repressed idea. And psychoanalysis, it is supposed, helps to effect a cure by aiding the patient to face the forgotten, repressed idea, and either to repress it more effectually, or else to avow and embrace it, whether in its original form or (in the case of sublimation) in some modified and improved form.

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[1] A contribution to the Symposium presented at the Joint Meeting of the British Psychological Society, the Aristotelian Society, and the *Mind* Association in Durham, July, 1914.

The notions of 'repression' and 'resistance' seem to offer a helpful and plausible means of picturing to oneself the course of hysterical and other neurotic troubles. Freud's language is highly figurative for the most part, and I am not quite sure how far he takes his picturesque, dynamic account literally, and how far he regards it merely as a working hypothesis, or even as a scientific fiction, for the purpose of colligating and describing the course of hysteria and kindred pathological conditions. Anyhow, having found these notions of repression and resistance helpful in dealing with pathological cases, Freud applies them also to the interpretation of the dreams, slips of memory, and other lapses, of normal people. The present discussion, I take it, is mainly concerned with the accuracy of Freud's conception of forgetting. The principal problem may be formulated as follows: Is the process of forgetting rightly conceived as a process of repression from consciousness followed up by a process of continued subconscious resistance, which confines the forgotten within the region of the unconscious, and prevents its emergence into clear consciousness?

It should be noted that I have employed the terms 'repression' and 'resistance' in different senses, not as synonyms. In this, I think, I have followed the Freudian theory more closely than Mr Pear has. Mr Pear seems to use 'repression' for both the initial repression and the subsequently continued subconscious resistance. For reasons which will appear presently I think it is important to discriminate between the two. One might accept the one without subscribing to the other, or, at least, one might be more confident about the one than about the other. Freud himself appears to be more sure of the 'resistance' than of the 'repression.' He treats the alleged fact of 'resistance' as a datum, while his belief in 'repression' is represented as an inference to account for the continued 'resistance.' My own view is the reverse of Freud's (if I understand him rightly). I have no hesitation in accepting the actuality of repression, but I do not think that there is sufficient reason for assuming a continued resistance. The phenomena of forgetting can, I believe, be sufficiently accounted for without our having recourse to the alleged resistance. Mr Pear, I take it, feels inclined to subscribe to the doctrine of 'resistance,' in the restricted sense in which it is here used, for that is what he appears to have in mind when he asserts, inter alia, that "there may be a semi-permanent inhibition preventing the revival of unpleasant memories."

The above distinction between 'repression' and 'resistance' is of considerable importance for the present discussion. Strictly speaking, 'repression' points to certain problems of attention, rather than to problems of forgetting. It concerns forgetting only indirectly, namely, in so far as attention and interest are among the most important conditions of memory. On the other hand, 'resistance' does relate primarily to forgetting, and indicates a new conception of the nature of that process. First, then, I propose to discuss 'repression.'

The process of repression is too obvious and too frequent an occurrence to have escaped the attention of psychologists altogether. Every book on psychology says something about it when discussing attention. Yet the usual account of it is hardly adequate. Most psychologists when dealing with attention are prone to accentuate reinforcement at the expense of inhibition. Inhibition is for the most part treated as a kind of negative consequence of reinforcement. A special expenditure of (psychical or neural) energy in one direction entails a draining of energy from other directions. Attention to some things necessitates inattention to others. To this extent it may be said, as indeed Mr Pear does say, that facilitation and inhibition are different aspects of the same process. I do not deny that many, perhaps most cases of inattention, inhibition, and arrest, may be accounted for by such a draining of energy. But I do not think that this is an adequate account of what for want of a better word may be called repression. The process of actively expelling a thought from consciousness, demands ampler recognition as a positive process, as something more than a bare falling out of consciousness on account of preoccupation with other matters. Even if we suppose that reinforcement and inhibition always go together, even so we should have to distinguish carefully between the case in which we fail to note *A* because we are fully occupied with *B*, and the case in which we concentrate attention on *B* because we do not want to think of *A*. One may feel inclined to describe this as a difference of stress; but it is an important difference all the same. Moreover, I do not think that the two processes of reinforcement and inhibition really always go together. Reinforcement, no doubt, always involves inhibition. But I think that there is such a thing as inhibition without reinforcement, repression without attention. We seem to have instances of this type of inhibition, or at least very close approximations to it, when, feeling tired or wishing to go to sleep, we try to repress upsurging thoughts without attending or wishing to attend to anything. My contention here is that side by side with attention, or the felt tendency to dwell on certain experiences or objects, we ought to recognise another process of the same rank, namely, 'repression,' or the felt tendency to get away from certain things or ideas—the two kinds of tendency being thus related to each other much in the same way as desire and aversion are related to one another. The popular psychology underlying everyday language does indeed recognise a difference between 'dismissing something from one's mind,' and merely 'failing to note or to recall it.' But the books on psychology have on the whole failed to do justice to the distinction, mainly, I suppose, because of the tendency to identify attention with consciousness. The Freudian movement may help to win a fuller recognition of the process of repression.

As here conceived, repression is primarily concerned with what is present to clear consciousness. According to its varying measures of success, the repression of an idea results in its removal from the focus of consciousness to the margin of consciousness, or to the sphere of subconsciousness, or of unconsciousness. If the objectionable idea is associated with many of our daily pursuits and interests, it may re-emerge in clear consciousness again and again, in spite of repeated repressions. When the associations are few, the repression will have a more lasting effect. The forgetting of the repressed idea is the result partly of the absence of spontaneous attention, which is necessary to secure the highest degree of retention, and partly it is the result of the absence of adequate reviving stimuli. Moreover, by repressing also ideas associated with the repressed idea, we may prevent them from reviving it even when they do occur to us. Ideas associated with an unpleasant, repressed idea are apt to produce a sort of derivative discomfort, so that we repress them before they have succeeded in completely reviving the objectionable memory. For example, if one has had an unpleasant experience in a certain town, he is apt to feel a vague discomfort as soon as anything relating to that place is mentioned; so that he will repress the suggested ideas before the memory of the distasteful experience has had a chance of being recalled. It is as though coming events had cast their shadows before them, and we took time by the forelock. But in all such cases the repression only affects what is actually present to consciousness. The forgetfulness, the failure to retain or to recall, I consider to be the negative result due to the absence of the necessary conditions of retention or of recollection.

Mr Pear, as already remarked, is inclined to agree with the Freudian view of forgetting as the effect of continuous, subconscious resistance. But the evidence adduced does not appear to me to call for such an hypothesis. It is, of course, impossible, in a short paper like this, to consider all the evidence. It seems to me that the Freudians sometimes mistake a patient's reluctance to *relate* his past experiences for a resistance to *recall* them. And some of the instances of apparent resistance to the emergence of a repressed idea look very like cases of the repression of associated ideas before they could re-awaken the repressed idea. Anyhow, at present I am only concerned with the evidence adduced by Mr Pear, and it may suffice if I just indicate how I would meet the various points in his paper in support of the theory of resistance.

(*a*) Mr Pear maintains that an idea or experience which has been so importunate as to require active swamping or repression cannot have failed to be remembered because of insufficient intensity, seeing that we commonly remember with great ease experiences not nearly vivid enough to have occasioned active expulsion. The forgetting, according to Mr Pear, must therefore be due to continued resistance. The argument would be cogent as against anyone who maintained that memory depends solely on the intensity of the original experience. But nobody regards intensity as more than one of the less important conditions of memory. And oblivion may be due to the absence of the other familiar conditions, not to the presence of resisting forces.

(*b*) The fact that the kind of things which people forget varies with their mental make-up, and the great ease with which people generally forget their dreams, even after recalling and recording them on waking, all this may be readily explained in the usual way, without having recourse to resistance. Anything closely connected with our permanent environment, or our habitual interests and pursuits, is best remembered and most easily recalled because of the abundance and frequency of reviving stimuli and associated ideas. Contrariwise, what is not so connected with our environment and habitual interests is not so well remembered or so easily recalled, simply because of the paucity or rarity of adequate, reviving stimuli. Hence both the kind of things that we remember and the kind of things we forget naturally vary with our mental make-up. Similarly with our dreams. In the first place, the absence of any real attention during sleep deprives dreams of an important condition of effective retentiveness. In the second place, there are such few incentives to make us recall our dreams, as they are usually not intimately connected with our waking interests and active pursuits, and do not stand the scrutiny of wakeful criticism. Even the waking consideration and record of a dream do not make it much more intimately associated with our waking interests, and we forget even waking experiences under such circumstances. Moreover, if the content of the dream is in any way unacceptable, this will lead to its repression, on waking, and so will indirectly facilitate its forgetting, in the way already explained above. But there is no need to posit resisting forces in our subconsciousness to account for the forgetting.

(*c*) The alleged convention by which people are held responsible for forgetting military and gallant services, shows, at the most, the existence of a device, or social sanction, for reinforcing attention to such duties and courtesies. It does not show that people believe that forgetting is caused by resistance, much less does it show that forgetting is really caused by the alleged resistance. I might remind Mr Pear of the familiar assumption that everybody knows the laws of the land—an assumption which everybody knows to be untrue. The bare absence of sufficient interest or attention to insure the remembrance of certain things, may be sufficient ground for offence, without supposing also the presence of a positive resistance because of dislike.

(*d*) The usual assumption that a healthy-minded person will try to forget an irreparable grief or disappointment, is sound enough. But this has reference to repression, not to resistance. And the recognition of repression, and even its indirect influence on forgetting, does not commit one in any way to the recognition of continuous resistance.

(*e*) Lastly, Mr Pear's experience of forgetting the name of his German friends does not appear to me to call for any other than the usual explanation. If it were really due to continuous resistance, as Mr Pear thinks it was, then it would be strange that the name of the dead friend, which was the principal object of the original repression and resistance, should have surged up in consciousness before the name of his German friends, which was a mere associate of the repressed idea, and therefore presumably not so strongly resisted.

My conclusions concerning the rôle of repression in forgetting may now be summarised as follows:

(1) Mr Pear uses the term repression so as to include two different processes which ought to be distinguished for the present problem. These may conveniently be distinguished and named in the following way. (*a*) *Repression*, or the process of expelling an idea from clear consciousness. (*b*) *Resistance*, or the subsequent process of continued retardation, or obstruction, by which the repressed idea is prevented from reappearing in clear consciousness.

(2) I recognise the actuality of the process of repression, in the restricted sense just explained.

(3) But I do not see sufficient ground for assuming the actuality of the alleged process of resistance, as defined above.

(4) Forgetting is, to my mind, sufficiently accounted for by the bare absence of the generally recognised conditions of retention and recall, so that the appeal to resistance is a gratuitous hypothesis.

(5) Repression (in the above-mentioned, restricted sense) promotes forgetting only in an indirect way, namely, by prematurely expelling associated ideas which might otherwise revive the repressed idea. For the rest, repression contributes nothing positive to induce forgetting. It is simply not favourable to recollection, or, more correctly, the conditions which lead to repression are such as do not favour memory.

THE RÔLE OF REPRESSION IN FORGETTING (III)[1].

By T. W. MITCHELL.

For the student of normal psychology the interest of the problems discussed by Mr Pear and Dr Wolf centres round the forgettings of everyday life. But we must not lose sight of the fact that the doctrine of repression was originally put forward by Freud as an interpretation of certain disturbances of memory which he encountered in the course of his investigations on hysteria. And since, as is well known, Freud's views have undergone considerable modification within recent years, it is important to bear in mind the changed implications which some of the terms he used at the beginning of his researches have gathered in the course of the general development of psycho-analytic theory. The need for being clear as to the meanings of the terms used in this discussion is well brought out by Dr Wolf's comments on Mr Pear's paper. Dr Wolf uses the words repression and resistance in a way which, although it suffices for the exposition of his own views, is rather confusing to those who are accustomed to the use made of these terms in the writings of the psycho-analysts.

At the time Freud formulated his doctrine of repression he held the view that the symptoms of hysteria are consequences of emotional shock in which strong excitement, that cannot have its normal outlet, is aroused. As a result of the blocking of the affective accompaniment of the experience, the ordinary associative mechanism is disturbed and the memory of the episode which caused the shock becomes split off or dissociated from consciousness so that voluntary recollection of it is impossible. An opportunity for 'catharsis,'—for the liberating of the pent-up emotion—occurs only when the memory of the shock and all its attendant circumstances can be brought back into consciousness.

In his very earliest work Freud used hypnosis as a means of broadening the field of consciousness and so recovering the lost memories; but when he found that many of his patients could not be hypnotized he had to rely upon other methods. Calling to mind how he had seen Bernheim bring back, in the waking state, the lapsed memories of deep hypnosis, by persistent urging and by assuring the patients that they could remember, Freud resolved to try the same plan with his neurotic patients whom he could not hypnotize. When he got to a point in the analysis at which the patient said he could remember nothing more, Freud told him that the correct memory would emerge as soon as he pressed his hand on the patient's forehead. He did not, of course, always succeed in arriving at the correct memory immediately by this method, but by persevering with it he was enabled to bring many analyses to a satisfactory conclusion.

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[1] A contribution to the Symposium presented at the Joint Meeting of the British Psychological Society, the Aristotelian Society, and the *Mind* Association, in Durham, July, 1914.

Freud found this way of conducting an analysis to be troublesome and exhausting. In attempting to drag up the unconscious memories into the consciousness of the patient, he got the feeling of exerting his own personal effort against some opposing force in the patient's mind. He says: "As this urging necessitated much exertion on my part, and showed me that I had to overcome a resistance, I therefore formulated the whole state of affairs into the following theory: through my psychic work I had to overcome a psychic force in the patient which opposed the pathogenic idea from becoming conscious[1]."

It was from his experience of this resistance to the emergence of pathogenic memories that Freud got the idea of repression. He thought that the force which caused the resistance in psycho-analysis must be the force which originally caused the forgetting; and this force he called 'repression.' On examining the nature of the forgotten ideas recovered in those analyses which he had already completed, he saw that they were all of a painful nature. They were memories of things which one does not like to experience and prefers to forget. Hence he regarded repression as consisting in the expulsion of those ideas from consciousness and the prevention of their return into consciousness, and he considered its occurrence to be a defensive reaction of the personality against unbearable ideas.

It was to this double function that Freud applied the term repression, and he thought repression was proved by the fact of resistance. At first sight it might seem that all repressed ideas are ideas that have been expelled from consciousness, and this is indeed the impression we get from the use of the word repression in Freud's earlier writings. But when we grasp the Freudian hypothesis of unconscious mental activity it becomes plain that many ideas which have never been in consciousness may be 'repressed' in the sense of being forcibly kept out of consciousness. And it is to ideas of this kind that the term repression seems mainly to apply in all recent psycho-analytic writings.

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[1] *Selected Papers on Hysteria*, p. 87.

The need for some distinguishing terms for the two separate processes,—the expulsion of ideas from consciousness and the prevention of the entry into consciousness of ideas already in the unconscious,—has long been felt; but no uniformity of usage seems to have been arrived at. Dr Wolf's proposal to confine the term repression to the process of expulsion from consciousness, and the term resistance to the prevention of their re-entry into consciousness, cannot be adopted by the psycho-analysts, for it would make unintelligible almost the whole of their writings of the past ten years. Perhaps the distinction most favoured by psycho-analysts is that of 'conscious repression' and 'unconscious repression.' The mere pushing of an idea out of consciousness is sometimes called 'suppression' (in a very different sense from that implied in Dr Wolf's use of this word), but suppression is not quite the same thing as 'conscious repression,' although it may be the first step towards it. The suppression of an idea consists in the pushing of an idea out of consciousness into what Freud calls the Foreconscious; its further penetration into the Unconscious and the keeping of it there is the work of repression.

As we have seen, Freud arrived at his conception of repression in the early days of his psycho-analytic work, and this conception was based on his experience of resistance,—on the expenditure of psychic force which his original *technique* entailed. This direct expenditure of psychic force by the analyst is to a great extent avoided in Freud's later methods. Instead of urging the patient to recall memories directly related to any particular symptom, the psycho-analyst of to-day relies on the free associations of the patient: for these associations, he believes, are not really free, but psychically determined, and lead surely, if slowly, to the sought-for memories. Resistance is not now felt as a psychic force against which the analyst has to struggle, but is revealed by the devious course which the associations take before the proper memories arise.

It seems to me that Freud's inference that resistance necessarily implies repression was not justified by the bare fact of this difficulty encountered in trying to resuscitate forgotten memories, and it seems open to question whether Freud would have come to the same conclusions regarding resistance and repression had he never used the 'pressure' method in psycho-analysis. It is true that in the method of free association, as in the method of urging, there is evidence of some hindrance to the emergence of pathogenic memories, but it is not so clear that this hindrance is due to anything that can rightly be called repression. Resistance due to repression implies some active opposing force which acts, as it were, from above. But there is such a thing as 'passive resistance,' a mere hindrance or obstruction, felt as resistance but necessitating no activity on the part of whatever causes the obstruction. Or what is felt as resistance may be due to a force acting from below as attraction or pull, rather than from above as repression or push. If a man falls into a pit it may need hard work to get him out, but the pull of gravity should not be described as repression although it is felt as resistance.

Now it is just to some psychical force analogous to the pull of gravity in the physical world that the main work of repression is ascribed by psycho-analysts at the present time. The amnesia of the hysteric or the forgettings of everyday life are not always due to the painful nature of the forgotten memories themselves, but to their association in the mind with other memories whose emergence into consciousness would be accompanied by pain. It is the attraction of the unconscious rather than the repulsion of the conscious that leads to the forgetting and causes resistance. So long as there is in the unconscious any nucleus of 'painful' memories which can exercise this attraction, those other memories that by any means have formed associative connexions with them will tend to be forgotten, and attempts to recover them will meet with 'resistance.' But the 'repression' here is due to a pull from below rather than to a push from above.

How, then, does the unconscious nucleus which exercises this attractive force come into existence? Always by repression, say the psycho-analysts. When the recent pathogenic memories of the hysteric are traced to their ultimate source they are always found to be in direct associative relation with certain childhood memories, which appear to have been repressed in deference to the ethical and cultural traditions into which the child was born. But having traced back these pathogenic memories to a period of childhood so early that it is hardly possible to suppose their relegation to the unconscious to have been due to anything of the nature of psychological repression, Freud was forced to postulate a prior phase or period of 'organic repression' in which, irrespective of any pressure from without, certain primitive instincts or impulses become latent.

Thus in the last analysis the process of repression,—and the process of forgetting, in so far as it may depend on repression—has its roots in normal organic developmental changes which are as independent of the psychical activity of the individual as are the visible physical modifications which his body undergoes. The early disappearance of these primitive instinctive impulses can no more rightly be called repression than the disappearance of the gill-slits in embryonic life can be so called.

Yet this so-called organic repression is regarded as the prototype and necessary prerequisite of all subsequent repression, and therefore of all the forgettings of everyday life that are claimed to be due to repression.

But the conscious recognition of the painful nature of the infantile memories that are forgotten in this period seems to play but a small part in the process of their 'repression,' and something other than the 'pain principle' would seem to be involved in the lapse of these memories into the unconscious.

There would seem to be here something of deeper import than a mere turning away from pain, something that pertains to a higher evolutionary principle than the pleasure-seeking tendencies of the individual life. There is involved what Mr Benjamin Kidd calls the principle of projected efficiency, through the disappearance of what would be detrimental to adaptation in the future. Usefulness for adaptation to a future environment is the most striking characteristic of all the developmental bodily changes that lead up to organic maturity, and we should expect to find the same principle in operation in the growth of the mind.

On this view we might regard 'organic repression' and the normal infantile repressions of a later period as being the outcome of an innate tendency to develop in ways that are conducive to future efficiency, rather than as the result of any conscious experience in the individual life of the pain which may accompany the giving way to ancestral impulses in the social environment of the present. And, on the surface at least, there would seem to be abundant evidence that this principle of utility continues to be effective throughout adult life in determining what shall be forgotten and what remembered. We systematize and organize our purposes, and we disregard all that does not seem to us to further them. We take an interest in and tend to remember what affects our purposes. What does not affect our purposes we take no interest in and tend to forget. It may be said that what interests us gives us pleasure, but it can hardly be maintained that what does not interest us gives us pain. And in everyday life it is just the things that do not interest us that seem to be most readily forgotten.

Thus 'repression' at all ages would often seem to depend on developmental changes which are accompanied by the formation of new interests, and the withdrawal of the older interests from consciousness by their absorption into the unconscious. It is only when the older interests of infancy persist alongside of and conflict with the newer interests which ought to have supplanted them, that the normal developmental process has to be reinforced by conscious repression. And it is just because the older interests conflict with the newer interests that they are now felt to be painful.

This is what is so frequently observed in those conditions of abnormal development which give rise to the psycho-neuroses. Psycho-analysis has shown unmistakably that conscious repression does sometimes play an important part in the forgetting of pathogenic memories. But as Jung admits "there are a number of cases where it is impossible for us to show, even with the most careful examination, the slightest trace of conscious repression; in these cases it seems as if the mechanism of repression were much more in the nature of a passive disappearance, or even as if the impressions were dragged beneath the surface by some force operating from below[1]."

This admission, by one of the foremost of psycho-analysts, that even the amnesia of the neuroses may have more than one mode of origin, should dispose us to regard with suspicion the generalisation of the ultra-Freudians who maintain that all forgetting, both normal and abnormal, is due to repression. In view of the many instances of forgetting of which the hypothesis of repression does not seem to afford any satisfactory explanation, and the *a priori* improbability that any one psychological mechanism will suffice for the interpretation of them all, we should perhaps be encouraged to examine again some of the psychological conceptions which have been rejected by the psycho-analysts,—both those that have been formulated by themselves and subsequently abandoned, and those that have been the outcome of the work of other schools of thought and other modes of interpretation.

As examples of some of the conceptions which have been perhaps too hastily given up I would mention the theory of dissociation put forward by Professor Janet, the actuality of those hypnoid states which Breuer postulated to account for some forms of hysterical amnesia, and the part played by emotional shock in the production of hysterical dissociation. Further, there would seem to be a most urgent need to reconsider the nature of post-hypnotic amnesia, and the broadening of the field of memory during hypnosis,—phases of consciousness which, so far as I am able to judge, are most inadequately explained on purely Freudian principles. Lastly the usefulness or uselessness of memories in the service of our interests and purposes would seem to deserve more consideration than it has received at the hands of the Freud school, in regard to the evanescence of dream memories and the forgettings of everyday life, as well as in the explanation of the origin of infantile repressions.

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[1] *The Psychoanalytic Review*, I. 9.

Nevertheless we must admit that the psycho-analysts have made out a strong case in favour of their interpretation of the amnesia of hysteria, and we must concede that forgetting in some cases at least, both normal and pathological, is most easily explained by repression (in the full technical sense of the word). But the evidence seems insufficient to justify the belief that all mental dissociation can be accounted for by repression, or that all forgetting has its ground or basis in the tendency to turn away from pain.

THE RÔLE OF REPRESSION IN FORGETTING (IV)[1].

By T. LOVEDAY.

I FIND it difficult to bring this discussion to an end otherwise than by silence because on the one hand the preceding contributions to it have in details dealt with somewhat diverse topics, whilst on the other hand they have all referred more or less directly to a problem which is far too large for treatment in this manner. The strictly Freudian conception of repression cannot be usefully discussed apart from Freud's other leading psychological doctrines, as Kronfeld quite clearly shows, and a general discussion of Freud's psychology is, I take it, not intended here.

On the whole the main question raised seems to be this, Whether there are special positive factors of forgetting distinct from the mere absence or contradiction of the usually recognized conditions of remembering.

If I understand Mr Pear's statements aright, he holds that irreparable decay of a memory is sufficiently accounted for by the falling out of some—more especially the physiological—conditions of retention, but that inability to recall at a given moment what is afterwards found not to have been lost irrecoverably may sometimes depend on special positive conditions—on psychical 'forces which prevent recollection.'

Now it may be that some memories are after a time not 'retained' at all and so are irrecoverably lost, but it lies in the nature of the case that this can never be more than conjecture. It is easy to show that there are many things which we do not succeed in remembering, but it is obviously impossible to show that we never under any conditions could recall them again. The discussion therefore inevitably leaves retention on one side, and centres upon recall.

It will be convenient to start from Dr Wolf's account of repression in the restricted sense which he gives to the term, as the volitional dismissal of a thought from clear consciousness. It cannot be doubted that this occurs, and in almost every point I find myself in agreement with what Dr Wolf says of it. There are three questions which, perhaps, deserve special consideration.

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[1] A contribution to the Symposium presented at the Joint Meeting of the British Psychological Society, the Aristotelian Society, and the *Mind* Association, in Durham, July, 1914.

(*a*) Is this repression ever a process of pure inhibition, as Dr Wolf suggests? I do not find this so in myself. Dr Wolf seems to state the nature of the process quite accurately when he says that "we concentrate attention on *B* because we do not want to think of *A*," except that it would be more precise to say that 'we want not to think of *A*.' Certainly I find in my own experience that the dismissal of the thought *A* always involves as a means attention to something else *B*. No doubt *B* is often, perhaps usually, not an object of thought: it may very well be a sensory experience. I notice that when in waking times I thus dismiss a thought I generally make a rapid but suddenly stopped turn of my head from that position which is natural to me in thinking to a rather forced and tense posture and that a number of muscles in the upper part of the body are held rather rigid; and the resulting complex sensory experience is very clearly apprehended. Even upon dismissing insurgent thoughts in order to go to sleep, I notice in myself attention to the sensations arising from respiratory movements and the like. No doubt, in what one may call fully developed instances of repression in this sense, one is aware that this attention to sensations is a means of escaping from the thought *A*, and one has also a curious feeling of a mental blank in place of *A*, similar to the feeling which is present in very unsuccessful attempts to recall anything. But I do not find that in this volitional repression this blank feeling is ever present by itself without attention to something else, and in more rudimentary instances of this state of mind it may be absent altogether, or at any rate extremely inconspicuous.

(*b*) Is this dismissal from clear consciousness always volitional, or is there, as analogy might lead us to expect, a lower (perhaps 'impulsive' or 'spontaneous') type in which *A* is expelled without express volition? We must, of course, exclude instances in which *A* simply yields place to the superior attractions of *B*; *B* must be attended to for the sake of getting rid of *A*. The question almost answers itself. There can be here no analogy with those processes in which a spontaneous may be distinguished from a volitional type, because if a thought *A* in clear consciousness be at once so insistent and yet so unpleasant or repulsive as to require dismissal, it must be so intimately related to self-consciousness that its dismissal cannot be other than a self-conscious act.

(*c*) Is repression in this sense of intentional dismissal a direct positive factor of forgetting? In particular, will *A* be more than usually difficult to recall simply because it has been thus dismissed from clear consciousness at some previous time? Dr Wolf's negative answer and his account of what does happen seem to me beyond dispute. But, of course, so far as one forms a habit of dismissing *A* and its associates from consciousness, a fair attempt at recall becomes more difficult.

I find it hard to understand in what sense exactly Mr Pear uses the term 'repression.' Sometimes he seems to refer to volitional expulsion of a thought from clear consciousness; at other times the repressed idea means one which cannot be recalled because of mental 'forces which prevent recollection.' But when Mr Pear uses the term in this latter sense, he leaves it quite vague how these forces are to be conceived. Does he, for instance, accept the doctrine of the censorship? Anyhow, I fully agree with Dr Wolf that the instances which he gives are quite insufficient to establish any new conditions of forgetting beyond those which have been generally recognized. I would only add that in my opinion the supposed conventional assumption or precept about forgetting griefs and disappointments is mis-stated. 'Forget' here means simply 'Don't brood over the matter,' perhaps 'Dismiss it from your mind for a time till you can view it in its proper relation to the rest of your life,' but it certainly does not mean 'Endeavour to be unable to recall it.' A man who really forgot that his wife or child was dead would be reckoned heartless, if not insane. Much the same is true of the precept 'Forgive and forget': only a fool would intend to forget entirely, however heartily he forgave. Even conventional maxims are not so idiotic as to forbid our learning and being guided by unpleasant experiences.

As I began by saying, Freud's conception of repression cannot be separated from the rest of his doctrine of mental behaviour. Freud's psychology is a marvellous collection of antique and modern curios, but he has packed them very tightly together. That he has discovered certain facts, or at least emphasized neglected facts, need not be denied, but in his explanations data, hypotheses and conclusions are muddled in one vast confusion. Dr Mitchell's presentation of the theory, so far as he agrees with it, conceals the paradoxes because he has selected so tenderly; but the result is to remove much of its novelty. Yet even Dr Mitchell has to speak of a "nucleus of 'painful' memories in the unconscious." The assumption seems to be that repressed ideas or thoughts are present and operative in unconsciousness just as ideas or thoughts. Indeed, I believe there is really implied in Freud's doctrine the astounding assumption that any idea or thought which can ever be recalled must be present as a thought in unconsciousness all the time. But the supposition that we are unconsciously thinking always and all at once of everything which we might under any conceivable conditions remember is only to be accounted for by the fact that Freud is thoroughly enmeshed in associationism and has not the very beginnings of a theory of judgment.

Freud seems to me to have established at least this about forgetting, that more often than is generally noticed our motives are mixed. That we may forget because we wish not to remember or because remembering would run counter to some other wish, and that even what we think an honest attempt to recall may fail because of such a wish, and that at the same time the wish may not be brought into relation to our self-consciousness—all that stands firm. But it all belongs to the same order of facts as the familiar fact that we may be in love without knowing it, and explanation is not really advanced by an elaborate doctrine of unconsciousness which is saved from patent absurdity only by the interposition of a fore-consciousness to do the troublesome work. Moreover, I doubt whether this kind of inability to remember is common in normal persons, though probably it is more frequent in the very sensitive, the very emotional, and those blessed with a strong "sense of sin" than in others. In any case it is pretty clear that most inability to remember is not of this kind in normal persons and is not due to repression. This seems to be admitted by all the contributors to this discussion, and I will note only three very obvious points.

(*a*) Probably in many persons whose memory is rather worse than the average, not merely in a particular direction, but all round, the defect is largely due to a tendency to live in the present and future and to neglect the past, which being thus but rarely repeated in memory becomes with lapse of time increasingly difficult to recall at will. If I may quote the instance best known to me, my own memory is poor all round, but I can discover no grounds for inferring frequent repression of unpleasant memories or frequent conflicts of the kind which Freudians regard as usual. In point of fact, among my vague and cloudy memories of childhood and boyhood it is mostly unpleasant experiences (of shyness, awkwardness and the like) that stand out relatively clear.

(*b*) Dr Mitchell's remarks on the relation of memory to our dominant interests seem to me very just, except that I cannot see what is gained by the assumption that our obsolete interests are 'absorbed into the unconscious.' (If that is the only way of saving Freud's assumption of the after-effects of assumed infantile sexuality, would it not be better to let all three assumptions go?) But the influence of present interests on memory has long been recognized, and indeed Dr Mitchell himself emphasizes it in protest against the narrowness of Freudian theory.

(c) Connected with this is the very obvious distinction, which Dr Mitchell states clearly but which the Freudians always tend to obliterate, between things that we don't specially care to remember and things that we definitely want to forget—between the merely uninteresting and the painful or repulsive. Now though it seems to me established that some fortunate persons can afford to forget because they wish to do so (consciously or sub-consciously), lack of interest is a much more important condition of forgetting than is negative interest. The uninteresting may, of course, mean that which did not interest us in original experience or that which would not interest us if recalled now. If we take it in the former sense, the fact that it is easily forgotten does not require any novel explanatory hypotheses. If we take it in the latter sense, we raise the whole question of relevance, both affective and logical. But there is no sense in saying that irrelevant thoughts which do not enter into consciousness at all are 'repressed' unless it can be shown that they exist—*i.e.* that we are thinking of such and such objects unconsciously,—and that is just what no one succeeds in showing. As to relevant thinking, if it admits of psychological explanation at all (as I think it does not), at any rate it is necessarily beyond the reach of the Freudian mechanism, which has no place for judgment.

THE PSYCHOLOGY OF MAGIC[1].

By CARVETH READ.

§ 1. *Antiquity of magic.*
§ 2. *What is magic?*
§ 3. *The beginnings of magic.*
§ 4. *How the belief in charms etc. originated.*
§ 5. *Magical force.*
§ 6. *Primitive ideas of causation.*
§ 7. *Magic and mystery.*
§ 8. *Personal magic.*
§ 9. *The evolution of magic:*
  Taboos, omens, dramatic magic; principles of mimetic and sympathetic magic; arbitrary elements in magic.
§ 10. *The dissolution of magic.*

§ 1. ANTIQUITY OF MAGIC.

MAGIC, until recently, was somewhat neglected by those who treated of primitive ideas. In Prof. Tylor's *Primitive Culture* only one chapter is given to Magic, against seven on Animism. In Spencer's *Sociology*, Part I, treating of primitive ideas, is wholly devoted to the genesis and development of Animism, without a single chapter on Magic. The importance of early Animism became such an obsession, that travellers observed and reported upon it wherever they went, making only casual references to Magic—much to our loss. The tradition of this way of thinking seems to run back to Hume's *Natural History of Religion*, where he traces the development of religion from a primitive belief that all natural activities are like our own, and that everything is possessed and actuated by a spirit. This idea was adopted by Comte, and elaborated in his celebrated law of the three stages of the explanation of Nature as determining the growth of human culture: Fetichism, which ascribes all causation to the particular will of each object, and which by generalisation leads through polytheism to monotheism; Metaphysics, which, giving up the notion of personal will, attributes the activities of things to abstract forces; and Positivism, which, discarding the variety of forces that can never be known, turns to the exact description of the order of phenomena. Mill, in turn, adopted these doctrines from Comte, and gave them currency amongst us, as part of the extraordinary influence which for many years he exerted upon all our thoughts. Hence the priority of Animism to every other theory of things seemed at that time a matter of course.

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[1] This article is an expansion of part of a paper read before the Royal Anthropological Institute, February, 1914.

Recently there have been signs that this conception of primitive thought is giving way to another, namely, that Animism was preceded by Magic; as Dr Frazer puts it, that an age of Magic preceded the age of Animism, as the palaeolithic preceded the neolithic age. His reasons are that the idea of magic is simpler than that of animism; that magic is found in full force amongst people whose animism is feebly developed, and that its beliefs are more uniform throughout the world[1]. Substantially—with qualifications that will appear hereafter—this view of the matter is accepted in the present paper. When one comes to argue it, to produce the primitive facts is, of course, impossible. It must even be admitted that such evidence as we have amongst the few facts collected of late years concerning ancient races of men, gives the earlier date to animistic ideas; for if some of the cave-paintings of Aurignacien origin, in which, for example, wild cattle are shown pierced with arrows, may be interpreted as of magical significance; on the other hand, the burial at Le Moustier, of a still earlier age, shows a regard for the corpse, in the disposal of it and in the things left with it, such as usually goes with the ghost-theory. These discoveries take us back some thousands of years before Menes; but probably leave us far from the beginning of human ideas.

If Magic preceded Animism, we must insert a stage of thought at the beginning of Comte's series, making four instead of three; and the suggestion may perhaps be made to appear plausible, that the Metaphysical stage, the reign of occult forces in explanation, is not a mere residue of Fetichism after the spirit has departed, but rather the re-emergence of magical ideas of force, that always persisted, but for ages were masked by the vogue of Animism.

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[1] *History of the Kingship*, p. 38; cf. also *The Golden Bough*.

§ 2. WHAT IS MAGIC ?

In his *Evolution of Moral Ideas* (II. 47) Prof. Westermarck observes that savages distinguish two classes of phenomena; the natural or familiar, and the supernatural or mysterious. The latter again are divided into the mechanical (Magic) and the volitional (Animism). This seems to be true: it corrects the notion, still common, that the savage explains all natural activities by Animism; recognises that he takes some phenomena as a matter of course, as the animals do, and rightly marks the distinction between the things that he does not take so as either mechanical (due to some uniformly acting force) or volitional (that is, arbitrary or capricious). With the savage, then, there are ongoings of things around him that are perceived to be regular and continuous; and there are others that are imagined to take place, and these either regularly or capriciously: for thus I venture to interpret the difference between the natural and the supernatural; it is the difference between that which is, or has been, perceived and that which is, and can only be, imagined—perception-belief and imagination-belief.

Magic may be defined as a connexion of events imagined to be constant and to depend upon the presence of some thing or activity possessing an efficacious quality or force (in fact unreal), and not to depend (as a connexion) upon any particular person.

Whether Magic is ever wrought by the bare wish or will of a human being will be discussed below (§ 8). Here, the proviso that a magical connexion does not depend upon a person, is meant to exclude Animism. For pure Animism involves the belief that a ghost, spirit, or god (though he may work by magic) can produce an effect by his direct action, without using any visible or invisible means other than his own spiritual body and force. This is not what a magician does: he works by means of a connexion of events known (so he thinks) to himself and often to others. The magical implement (talisman or spell) that he uses has qualities that are magical facts, just as the qualities of his spear are physical facts. He can make a stone spear-head by means of another stone; and he may be able to make a talisman by means of a spell; but the powers of the talisman or of the spell are their own; he cannot create Magic, but only discover and use it. Whether he shall use it, depends upon his choice; but its powers do not; they are inherent, like physical forces. Faust can conjure the devil; but so can Wagner, if he knows the spell: the power of the spell is indifferent to the conjuror.

A rule of Magic, as describing an uniform connexion of events, resembles what we call a law of nature; and further in this, that it is not supposed to be absolute or unconditional, but a tendency, subject to counteraction by hostile magic or (perhaps) by demonic force.

To practise Magic is to use some such rule in order to obtain an end desired. This can be attempted by any man, so far as his knowledge reaches; and the greater his knowledge the greater his power, provided he have the courage to act upon it. All stages of proficiency may be traced from the simple layman who swings a bull-roarer to raise the wind, to the wizard who lives by his art, is feared by all his tribe and far beyond it, or to the erudite magician who controls the demons and vies with the gods.

Magical things, objects or actions, in their simplest forms, are charms, spells, and rites; and since the ends for which they can be used are either to protect oneself or to exert power over other persons or things, each of these kinds of magic-thing may be defensive or offensive. A defensive charm is called an amulet; an offensive charm is a talisman. For a defensive spell (say, against sickness or accident) there is, I believe, no appropriate name; offensive spells (say, to control the weather or to curse an enemy) may be called incantations (but the usage is not fixed). Rites (that is, any magic actions that are not spells) may also be defensive (as to touch wood), or offensive (as to point at a man); but we have no names for these different intentions. From these simplest beginnings the whole learning and mystery of Magic seem to have been developed.

§ 3. THE BEGINNINGS OF MAGIC.

To discuss the beginnings of Magic must necessarily be, in some measure, a speculative undertaking; because the facts are lost. If Magic was practised in the Aurignacian culture (say) 20,000 years ago, how can we get to the back of it? But speculation is not guess-work, if we always keep in view such facts as we have; if we are careful to give notice whenever the facts fail us; if we guide ourselves by scientific principles; and if we make no assumptions merely to suit our case, but only such as are generally admitted in all departments. For example:

(1) We may reasonably assume that the simplest magical beliefs

and practices are of the earliest type: and nothing can be simpler than the belief in charms, rites and spells. It is, indeed, difficult to find many of these practices—though some can be produced—in their simplest forms in our records of backward peoples; partly because they have not been enough observed; partly because Magic is apt everywhere to become saturated with Animism; partly because charms, rites, and spells are generally, for greater efficacy, compounded with one another. But these considerations do not affect the simplicity of the idea involved in the magical beliefs; which is merely this, that a certain object by its presence, or that an action, or an utterance, by merely entering into the course of events, will serve our purpose. A bare uniformity of connexion—if A, then B—in analogy with the 'familiar' ongoings of nature and our common activities, is all that is assumed. Many kinds of obstacles stop an arrow or a dart; carrion collects the vultures: so a patterned comb in one's hair, stops the demon of disease; a patterned quiver, or a certain song, brings the monkeys down from the tree-tops[1]. A spell assumes merely that certain objects or animals will comply with a wish expressed in words, just as another human being often does. If any form of words ever seems to have been successful, it must be repeated; because to have the same effect the action must be the same. Immersed in an indefinite mass of experiences, the postulate that we call 'cause and effect' (unformulated, of course) underlies every action, and therefore underlies Magic; it is the ground of all expectation and of all confidence.

(2) The types of Magic that are the most prevalent are probably the earliest; and these are charms, rites and spells. They are found not only amongst savages, but the world over: even in civilised countries most of the uneducated, many of the half-educated, and not a few of those supposed to have 'finished their education,' employ them. What is universal must be adapted to very simple conditions of existence; these beliefs, therefore, to mental wants and proclivities that are probably primitive; and what conditions can be more simple or more primitive than ignorance of our fate and eagerness to clutch at anything that may give us confidence?

And not only are charms, rites and spells in all ages and everywhere employed in their simplest forms; but analysis of the most elaborate ritual and of nominally animistic practices will discover the same beliefs at the bottom of them. The idea of the amulet or the talisman is found in fetiches, beads, praying-wheels, and equally in a long mimetic dance or a passion-play; whose central purpose always is to avert some evil or to secure some good—success in hunting, or war, or agriculture. Similarly, the spell is involved in all rites, so far as verbal formulae are used in them; and in all curses or prayers, so far as their efficacy depends upon a sound form of words.

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[1] Skeat and Blagden, *Pagan Races of the Malay Peninsula*, I. 417.

The persistence of magical beliefs amongst civilised people is, in some measure, due to tradition; though the tradition could not continue effective if there were not in every generation a predisposition to accept it; and how far it is due to tradition, how far to a spontaneous proclivity, in anyone who is inclined to believe in charms, rites and spells, he himself must judge as best he can. So much of one's early life is always forgotten that no one can be sure that he was not inoculated with such notions in childhood; especially when we consider that religious practices, taught before they can be well understood, must often wear a magical habit to a child; and that medicines, whose operation nobody used to understand, were on the same foot with Magic. For my own part, baiting the last considerations, I cannot remember ever to have heard in childhood any sort of Magic spoken of except with amusement; yet magical beliefs have always haunted me. With Animism it was otherwise. When six or seven years old, I was told by a nursemaid—a convinced adherent of one of the many little sects that have ramified out of Wesleyanism in Cornwall—the most appalling ghost-stories; and these stories, exciting no doubt an ancient disposition, and reinforced by a visualising faculty that nightly peopled the darkness with innumerable spectres, entirely overpowered the teachings of those whom there was better reason to trust, that ghosts were a superstition from which true religion had for ever set us free. The effects lasted into middle age. Andrew Lang said of ghosts that 'he did not believe, but he trembled'; and that precisely describes my own state of mind for many years. Now, as to Magic, my impression is that had it been suggested to me in a serious way, and not merely by casual allusions to 'luck,' the experience would have stuck in my memory. The first time that I can remember practising rites, I was between ten and eleven years old, living at a small boarding-school. To keep off a dreaded event, I used to go every morning to the pump, fill my mouth with water and spurt it out in a violent stream—three times. This went on for some weeks. To the best of my recollection, the impulse was spontaneous, and I cannot remember why the particular rite was adopted. Nothing further of the kind recurs to me until the age of fifteen, when, at another school, the master-passion of my life was cricket;

and I always practised some rite before going on the field, and carried a charm in my pocket, but I cannot recall what they were. At present, a day rarely passes without my experiencing some impulse to practise magic. In lighting my first pipe in the morning (for example), if I remember how I lit it, where I struck the match, etc., yesterday, and if no misfortune has happened since, I feel an unmistakable impulse to 'light up' again in the same way. No reason is distinctly present to me for acting so, and therefore it is hazardous to analyse the attitude; but it is not merely incipient habit: to the best of my judgment it is this, that such a way of lighting my pipe was one amongst the antecedents of a quiet time, and that it will be well to reinstate as many of them as possible. It is more comfortable to do it than to alter it: one feels more confident.

Inquiry amongst my friends shows that some have similar experiences, and others (both men and women) have not. A *questionnaire* on such a point might be useful; but difficult to arrange without giving suggestion or exciting bias.

§ 4. HOW THE BELIEF IN CHARMS, ETC., ORIGINATED.

(3) There are causes of the belief in charms, rites and spells so simple that nothing could be more natural to the primitive human mind. What is more universally powerful in producing belief in the connexion of events, and consequent expectancy of repetition, than an interesting coincidence? If, says Sir E. F. im Thurn[1], a Carib sees a rock in any way abnormal or curious, and if shortly after any evil happens to him, he regards rock and evil as cause and effect, and perceives in the rock a spirit. This is animistic; but the same tendency to be impressed by coincidence underlies Magic. For example: A hunting party of Eskimos met with no game. One of them went back to the sledges and got the ham-bone of a dog to eat. Returning with this in his hand, he met and killed a seal. Ever afterwards he carried a ham-bone in his hand when hunting[2]. The ham-bone had become a talisman. Such a mental state as the Eskimo's used to be ascribed to the association of ideas; but it is better to consider it as an empirical judgment. Other circumstances of the event may have been associated in his mind; but as to the ham-bone and the kill, he thinks of them together, and judges that they are connected. Then, having judged

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[1] *Among the Indians of Guiana*, p. 354.

[2] Quoted by Prof. Ames, *Psychology of Religious Experience*, p. 60.

that the ham-bone influenced the kill, he carries it with him in future in order to repeat the conditions of success.

That Magic should depend upon the assumption that 'things connected in thought are connected in fact' seems unintelligible, until we consider that the thought in which they are connected is a judgment concerning the facts. But how is it possible to judge so foolishly? Absurd association is intelligible; it cannot be helped; but judgment is not a passive process. Well: a logician might interpret the case as a fallacy in applying the inductive canons: the Eskimo had two instances: (*a*) no ham-bone, no seal; (*b*) ham-bone, seal: or, again, into the circumstances of his expedition he introduced a ham-bone, and the seal followed. He had not read Mill, and did not know what precautions should be taken before adopting conclusions. The mind must have begun in this way; and after many thousands of years, the opportunities of error in empirical judgment began to be appreciated, and the canons were formulated. The experience of every man at every stage of life, personal or social (probably evoking an inherited disposition), continually impresses him with the belief in some connexion of antecedent and consequent, that each event arises out of others. But what it is in the antecedent that determines the event, in practical affairs, can rarely be exactly known. In definite cases, where method is applicable, we may analyse the consequent into the tendencies of forces in amount and direction; or we may sometimes reproduce it by an experiment exactly controlled. But where these resources fail us, as they often do in practical affairs, we are little confident of grasping all, or even the chief conditions of any event that interests us. The savage knows nothing of method, and, therefore, feels less distrust. He knows something of the conditions of success in hunting; this is biologically necessary to him as it is to a tiger; only those survive that learn them. He knows that he must have weapons as efficient as possible, must go to the habitat of his prey, must not be heard, or seen, or smelt. But with all precautions he sometimes fails; therefore there must be some other condition which he does not understand. Hence anything observed, or any word or action, that happens to be followed by success, may have been a means to that success.

And if it once has been, it always will be: not that he thinks of it in this general way. But even animals, as soon as they have learnt a sequence, trust to it. Dogs and cats learn faster than guinea-pigs; monkeys faster than dogs and cats; man quicker still. But in no case would there be any use in learning, if it did not lead to action as if the experience had been generalised. There is no mystery in generalisation;

it is spontaneous, and only waits for language to express itself; the difficult thing to acquire is caution. Primitive Magic is an incautious, unexpressed generalisation; and the conditions are such that the error cannot be easily detected. As long as a savage follows the instinctive, traditionary and acquired knowledge that he has of hunting, practices based on wrong judgments, but not interfering with the traditionary art, such as the carrying about of a bone, or a crystal, or having a special pattern on his quiver, cannot impair his success; may add to it by increasing his confidence: so that as long as success lasts there is nothing to suggest the falsity of the judgment. A series of failures may make the hunter throw away his talisman, or (at a later stage of Magic) take it to a medicine-man to be redoctored. How many thousands of times may Magic have begun in this way and lost its hold again, before the human mind became chronically infected with it!

Similarly with amulets, anything unusual that a man happens to have about him, when attacked by a leopard or snake, from which he escapes, may be kept as a safeguard against future perils. And with rites: any gesture or action, however irrelevant, that happens to precede a successful effort, may be repeated in order to reinstate all the antecedents of success. So, too, with the origin of spells. It is a common impulse, and quite spontaneous, to accompany an action with words, incentive or expletive. If a hunter does so in driving home his spear successfully, he will next time repeat the words: they then become a spell[1].

In trying to imagine ourselves in the place of such a man, we must not omit the emotional excitement that determines his judgment. It is the intensity of his desire for success, his anxiety about it, that makes him snatch at, and cling to, whatever may possibly be a means to it: he does not see how it acts, but will take no risks by omitting it. Since what is true of primitive hunting is true of all undertakings—that we know some conditions of success but far from all—and since we are all of us frequently in this position—the wonder is that we resort so little to Magic. That many people do so in London is notorious.

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[1] Charms (and possibly rites and spells) are sometimes revealed in dreams (Howitt: *The Native Tribes of South-East Australia*, p. 378; and Hose and McDougall: *Pagan Tribes of Borneo*, I. 110). But this can only happen either where the belief in charms already exists (as in the cases cited), or by the coincidence of the dream with good or bad fortune. The connexion of events must first of all present itself as something observed: whether waking or dreaming is indifferent.

§ 5. MAGICAL FORCE.

Magic is an uniform connexion of events, depending on some impersonal force that has no real existence. What causes make men believe in such force? In the first place, everything is necessarily conceived of by everybody as a centre of forces. Its weight, when we try to lift it; inertia, when we push it, or when it is moving and we try to stop it; degree of hardness, when we strike or grasp it; elasticity, when the bent bough of a tree recoils; the sway of torrents and the lift of waves; falling trees, avalanches, water-spouts: all these things a man must think of in comparison with his own strength or impotence; and as their action is full of surprises he does not know all that they can do.

But, further, the strain exhibited by some objects, especially trees; the noises they make, creaking and groaning in the wind; the shrieking and moaning of the wind itself, thunder and the roar of torrents: all excite *Einfühlung* or empathy, illusory sympathy with things that do not feel. Their voices, more than anything else, I believe, endow them for our imaginations with an inward life, which Mr Marett has well called Animatism[1], and which must not be confounded with the Animism that used to be attributed to all children and all savages—the belief that every object is actuated by a spirit, or even that it has a consciousness like our own. What truth there was in that doctrine is fully covered by Animatism.

Again, the savage is acquainted with invisible forces that seem to act at a distance. The wind is the great type of invisible force; heat, sound, odour are also invisible, and act at a distance; the light of the camp-fire acts at a distance, spreading across the prairie or into the recesses of the forest. In dreams, one visits some remote hunting-ground and returns instantly; distance and time are distorted or annihilated.

Hence there are abundant analogues in experience by which the savage can conceive of his talisman as a force-thing that acts invisibly and acts at a distance. Australian medicine-men "threw their joïas (evil magic) invisibly, 'like the wind,' as they said[2]." The talisman acts at a distance, like a missile; it acts for the person who owns it as his spear does, and is dangerous to others who have not practised with it. Sometimes, indeed, magic things have no owners, and are dangerous to everybody: malignant influence radiating from them, like the heat of a fire or the stench of a corpse. Such are two stones described by Spencer and Gillen[1] as marking the place where an old man and two women died for breaking a marriage-taboo. "They are so full of evil magic that if any but old men go near, it kills them. Now and then a very old man goes and throws stones and bushes upon the spot to keep down the evil magic." It is as if one covered up a fire or a corpse. And, further, magical force may exist in a diffused way, like darkness, heat, cold, epidemic disease, tribal unrest, without necessarily attaching to any particular thing. In the Western Isles of Torres Straits[2] mishaps are signs that something has gone wrong, or will go wrong, elsewhere. A fisherman, generally successful, having once failed at his task, was depressed; but on two women dying in his village soon after, he was consoled; since this showed that failure was not his fault. From this way of thinking it is but a step to the conception of *mana, manitou, wakanda.* And there may be currents of this magical force, favourable or unfavourable to things in general; as may be seen in the flow and ebb of the tide, the waxing and waning of the moon, and in the course of the seasons.

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[1] R. R. Marett, *Preanimistic Religion*, *Folk-Lore*, 1900.

[2] A. W. Howitt, *The Native Tribes of South-East Australia*, p. 371.

Rites also act at a distance by invisible force; for this power belongs to gesture-language, and the simplest rites are gestures. And so do spells; for wishes, commands, threats expressed in words, act upon men at a distance by invisible force; and spells are nothing but such expressions conceived of in a particular way, as having that uniform efficacity which is Magic. Spells, being thought of as forces, are reified; so that blessings or curses cling to their objects like garlands, or like contaminating rags.

§ 6. PRIMITIVE IDEAS OF CAUSATION.

The postulate that all the ongoings of the world may be analysed upon the principle of cause and effect, can never come into the minds of primitive men; and even to ourselves it rarely occurs, except in scientific discussions or logical exercises; but we always act as if we trusted in it, and so does a savage, and so does a dog. This ungeneralised presumption determines all thoughts and actions, mechanical, animistic, or magical. In our modern analysis of mechanical causation there are four requirements: (1) everything has a cause; (2) causation is uniform; (3) the cause is the assemblage of the indispensable conditions of the effect (and no others); (4) the cause is proportional to the effect. In the savage's mind these requirements are also obscurely recognised: (1) he seeks a cause for everything: (2) he expects it to act uniformly: (3) he believes in reinstating all the conditions of the effect, as appears from his preparations for hunting, or war, or marriage, or whatever he may have in hand; but he is much more ignorant than we are as to what conditions are indispensable and decisive: (4) he knows vaguely that the cause is proportional to the effect; for (*a*) he succeeds in making effective weapons, etc., by mechanical means; (*b*) he is very susceptible to the 'size-weight illusion,' twice as much as we are[1], and this implies the adjustment of effort to a supposed weight; (*c*) he repeats a spell in order to strengthen it, or unites a talisman, a rite and a spell in one assault, implying that greater force must be directed against greater resistance; and so on.

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[1] *Northern Tribes of Central Australia*, p. 472.

[2] A. C. Haddon, *Reports of the Cambridge Expedition to Torres Straits*, v. 361.

Causation, then, is universally assumed: how do Animism and Magic influence the latent conception? As to Animism, (1) the ghost is at first merely a man-force disengaged from the visible body, but still having its own body through which it can produce various effects. But the fear of a ghost immediately endows it with superhuman power; and in course of time it becomes more and more powerful, even to omnipotence; so that it can of itself bring about any event; and, therefore, the 'assemblage of conditions' becomes unnecessary to causation: there need be no visible conditions: miracles occur; creation out of nothing is possible. (2) For such a power the 'proportionality of cause and effect' becomes almost meaningless; but not quite, for there are degrees of spirit-force; and what one cannot do a stronger can. (3) The uniformity of causation is impaired; for though the ghost or spirit acts from human motives, no one knows exactly what its motives are. It may be cajoled, implored, even threatened; but the result does not always answer desire: there is apparent caprice, with consequent loss of confidence, and an uncomfortable feeling that leads to a reaction in favour of controlling the god, perhaps by Magic.

As to Magic, it has the great merit of (1) preserving the uniformity of causation. But (2) it impairs the presumption of causation by including in the total antecedent unreal conditions, namely, the forces of rites and spells; and by generating a frame of mind that makes it impossible to eliminate them. The hunter uses all the resources of his art in hunting; he also practises rites and spells. To which does he owe his success? We might suggest that he should try the hunting without the rites; but he is afraid to: and should he try, his positivism would probably break down at the first failure to kill. Well, then, let him try the rites without the hunting; but he is not such a fool: and, if he were, the medicine-man would tell him that, of course, he must 'use the means.' (3) Magic impairs the sense of proportionality between cause and effect, by recognising antecedents which are, in their nature, immeasurable. In their admirable work, *The Pagan Tribes of Borneo,* Messrs Hose and McDougall remark (Vol. II. p. 2) that it is sometimes said, that people of lowly culture have "no conception of mechanical causation, and that every material object is regarded by them as animated"; but they do not think that this could be truthfully said about any of the peoples of Borneo. In the construction of houses, boats, weapons, traps, they have a nice appreciation of the principles involved. Yet we find (at p. 124) that these skilled artizans believe that they can retard an enemy's boat by hanging under one of its benches a quartz pebble. Similarly the Boloki, having made a good canoe, before launching, strike it on the stern with an axe "to take away the weight[1]."

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[1] W. McDougall, *Reports of the Cambridge Expedition to Torres Straits*, II. 199.

If there is any truth in the foregoing account of the latent ideas, or presumptions, of savages concerning causation, it is plain that Magic (and likewise Animism) did not help, but hindered the development of these ideas. Notwithstanding (or, perhaps, because of) certain specious resemblances to science, Magic is, and always has been, the enemy of science.

§ 7. MAGIC AND MYSTERY.

Magic begins with ignorance of some of the conditions of a desired event, and the adoption (on account of coincidence) of anything that fixes one's attention as contributing to the total antecedent. As the supposed conditions of events grow more and more miscellaneous, the disposition increases to regard anything as a possible cause of anything else. Hence unbounded suspicion whenever an interesting event occurs whose antecedents are not familiar and manifest. The more unusual any occurrence, the more it must excite attention and be apt to arouse suspicion; and the first arrival of travellers or missionaries amongst a wild tribe is an event so unprecedented, that it is likely enough to occasion exaggerated suspicions and behaviour which, when reported, misrepresent the tribe's normal attitude of mind. For this allowance should be made.

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[1] Weeks: *Among Congo Cannibals,* p. 311.

However, in all Magic the causation is not traceable; therefore it becomes mysterious. The sense of mystery arises when something excites wonder, wonder gives place to curiosity, curiosity is baffled, and wonder returns with fear. The magical power of an inert object, such as a pebble or shark's tooth, or of a bone that is pointed toward a victim, or of a spear that is swung but not thrown, and nevertheless inflicts a wound, though an invisible one, is something unknown, or mysterious, or (as Mr Marett says) 'awful.' That is to say, it is a merely imagined action which remains obscure and inscrutable, in comparison with the perceived action of a spear thrown at a deer and slaying it with a visible wound. The savage has adequate practical knowledge of the latter process; of the former only a vague analogical image. He feels the difference, and that the magic is mysterious; and mystery, being common to all magical processes, and deeply impressive (especially in black magic), becomes what we call a 'fundamental attribute' of magic, pervading the whole apperceptive mass that is formed by a man's magic-beliefs. Therefore whatever excites the sense of mystery tends to be assimilated[1] by this apperceptive mass and confirmed as magical; just as the mass of our scientific knowledge assimilates and confirms any proposition that has the attributes of a scientific law. Hence the savage accepts the mysterious remedies of an European doctor as magical; and he is ready to regard as magical, and to fear, anything—a rock, tree, or water-spout—that by any trait excites his sense of mystery. Magic, then, does not arise from mystery; but having come into existence, it appeals to the feeling of mystery; and then whatever else is mysterious tends to become magical[2].

Magic being mysterious, the more mysterious the more powerful it must be. Hence foreign magic is more dreaded than the home-made; ancient magic is more powerful than modern; muttered spells, or formulae in a strange language, or in one whose meaning is lost, are more efficacious than intelligible speech; verse is more subtle than prose (for poets are everywhere respected); a wizard is more impressive in a mask than when bare-faced, in a fit than when sober; and operations in the dark, that ought to excite scepticism, increase credulity.

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[1] *Tends to be* assimilated—for if the presentation has some special character of Animism, it will be assimilated to the animistic system.

[2] It has been thought strange that such a thing as a whirlwind may excite in the savage either fear or anger. To explain this we must consider the nature of wonder: it is an imaginative expansion of surprise, temporary paralysis of the imagination, with emotional disturbance, but no progressive instinct of its own. It either subsides helplessly, or gives place to curiosity, or passes into some other emotion that is connected with an instinct. Accordingly it usually passes into curiosity or else fear, but sometimes into anger: which of these emotions shall be aroused depends, partly, upon the character of the person who wonders, partly, upon circumstances.

Masses of ideas having this mysterious quality form dissociated systems, which offer resistance to all ideas that want it, and therefore to what we call 'explanation.' Hence the conservative, inexpugnable character of magic and its easy alliance with mysticism. Resistance to explanation may go the length of denying present experience; as when a man is seen to be slain by a weapon or by a falling tree, and yet his death is ascribed to sorcery: the fear of sorcery having become a fixed idea or dissociated system. Surely, the occurrence of tribal insanity is indisputable.

§ 8. Personal Magic.

Magical power inheres in things, rites and spells without regard to the man who uses them; they may be sold or taught, and then serve the new possessor. But much the greater part of all known magical operation depends upon the agency of some person, man or spirit, who sets it going; and rites and spells are from the first, by their nature, personal actions.

In the later stages of Magic, no doubt, it is held that a mere wish or volition may be efficacious: in Cornwall they still tell you, when you are cold or tired, that you 'look wisht,' that is, as if you had been ill-wisht, and were pining away (this meaning is generally forgotten). But it seems to me improbable that such an idea should be primitive. People who, in inflicting penalties, do not discriminate between accidental and intentional offences, cannot be supposed to ascribe power to a mental process as such; and I have not been able to find in reports of very backward tribes any case of wish-magic unsupported by magical implements. Dr Haddon says[1] that, among the eastern islanders of Torres Straits, the power of words and the projection of the will were greatly believed in. A youth who makes love procures a piece of black lava shaped significantly, and anoints it with coco-nut oil, etc.; he also anoints his own temples, and thinks as intently as possible about the girl, and repeats a spell whenever he sees her, using the names of

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[1] *Reports of the Cambridge Expedition to Torres Straits*, VI. 321.

Sagaro and Pikaro, wife and mistress of the hero Sida. Now, with this complex apparatus of talisman, rite and spell, how much is left to wishing or the projection of the will? He thinks hard about the girl, no doubt; but that needs no voluntary effort: at least, in this climate, voluntary effort is soon exhausted in trying to think of anything else. Mr Weeks, in his book *Among Congo Cannibals*, describing a people of somewhat higher culture, and much more advanced Animism than the natives of Torres Straits, explains the nervousness in the poison ordeal even of those who are innocent of any nefarious practices, by asking—"For who has not at some time wished another's death?"—and by the admitted doctrine that a man may be full of evil magic without knowing it. From this it seems to be inferable that a mere wish may be effective. But not justly; for a man full of evil magic is an incarnate talisman; and such a man may operate without wishing, as often happens with the evil-eye. At any rate, these casual Congo wishes do not amount to volition; the faintest impulse is enough for the hair-trigger of such explosive personalities.

Still, with the progress of Magic, wishes may acquire independent power. A man practising evil magic, and desiring secrecy, has a strong motive to believe (that is, there is a cause of his believing) that his tacit wish has the power of invisible action at a distance; and the way by which he arrives at that belief is quite clear: for a spell is a magical force; it is also an expressed wish or command; and, if muttered, is more rather than less powerful. Why not, then, 'sub-muttered,' or merely thought?

From this degenerate notion of will-work we must distinguish the personal power of an accomplished wizard in his whole magical activity. Whatever a man does with a talisman or a spell, as it were with a tool or a weapon, may be done with more or less concentration of energy and proficiency in the performance; and one man will be plainly superior to another. Success depends upon doing one's best; therefore upon the will. And Magic often needs courage and resource; but in the development of the art it depends still more upon knowledge; wherein the magician is wont to boast himself. But to discuss the character of the magician would need a chapter or a volume.

§ 9. THE EVOLUTION OF MAGIC.

(*a*) From the simple beginnings thus described, Magic, like everything else in the world, proceeds to grow and differentiate. It grows by the lengthening of spells from the briefest wish or command to many verses[1]; by the lengthening of rites, until they last many days; by the accumulating of charms in bags-full; by the compounding of rites with spells, and by repetition. It differentiates by being extended and adapted to more and more purposes: from hunting (if we suppose it to begin there) to war and love; to birth, marriage and death; to the giving of diseases and the curing of them; to the protection of property and to the discovery, or the concealment, of theft; to navigation, building, agriculture, renewing the vigour of the sun and binding the influence of the Pleiades. At the same time, Magic, originally practised (we may presume) by individuals, comes to engage the concern and cooperation of families, clans and tribes. And a distinction grows up between what we call 'white and black magic': practices which are for the welfare of the tribe or some portion of it, and practices designed to gratify private passions without regard to their effect upon the community.

(*b*) One highly influential factor in the development of Magic is its alliance with Animism; but that requires separate consideration.

(*c*) Important outgrowths of primitive Magic are taboos. Taboo is the dangerousness of a person or thing conceived as a motive for not touching or meddling therewith. The dangerousness may be either in the nature of the person or thing, or imposed upon it. A chief, for example, and everything belonging to him, is generally taboo by his inherent sacredness. He is, like Mr Weeks's Congolese, full of evil magic: the idea of the talisman has thus been extended to include certain men. In many tribes it also includes all women; but since to make them always taboo is too much for human nature, they are treated as such only periodically, or when a man is about to be exposed to some further danger, which will be the more likely to injure him when already contaminated by evil magic[2]. This is very clear in a case reported by Prof. Seligman[3]: at Yam warriors were forbidden to sleep with their wives before battle; else "bow and arrow belong other fellow he smell you, he shoot you, you no got luck." Still, physical consequences may also be considered: a diver for pearl-shell must also abstain; because, else, "man he sleepy." The continence required of women when their husbands are away at war or hunting is a different matter. It is usually put down to 'sympathy' or 'participation,' as if husband and wife were somehow identical; but when magical taboos are generally recognised and feared, it is possible to use them for social purposes. Thus the food-rules of Australian tribes are plainly designed in the interests of the elders; so why should not a fiction be founded upon Magic in the interests of absent husbands?

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[1] One may trace this process in the admirable collection of spells in Mr Skeat's *Malay Magic.*

[2] See *The Naga Tribes of Manipur* by T. C. Hodson, p. 88.

[3] *Reports of the Cambridge Expedition to Torres Straits*, v. 271.

Taboos are also imposed upon things otherwise harmless, usually by declaring a curse upon anyone who meddles with them. This is the common case of making a talisman by means of a spell, transferring the invisible power.

The sacredness or dangerousness of the chief is probably due, first, to his being really dangerous; but, secondly, to the biological advantage to a tribe of respecting him, which leads to a selection of tribes in whom respect for the chief arises. As biological adaptation is never more than a moving oscillating equilibrium, this feeling may sometimes become excessive, even to the point of insanity. The dangerousness of women is attributed by Prof. Westermarck to horror of blood; very probably; but other things may cooperate toward it. The life of women separates them from men, and brings them into a community of interests, and differentiates their mentality, until the result is mysterious and therefore magical; and the sexual orgasm, being more like pain than anything that is not pain, at once attractive and revulsive, acquires the same character.

(*d*) Omens are amongst the antecedents of an interesting event; and, on the principle that ideas are differentiated from a confused matrix, it is probable that they were not originally distinguished from the other antecedents or *quasi*-mechanical conditions; but, as omens, they are merely signs, not causes. If we compare omens with mechanical and *quasi*-mechanical conditions, several differences may be detected, which may have gradually separated them in the savage mind. First, omens need no human intervention; whereas rites and spells must be performed, and charms are usually carried about one. Secondly, many omens convey no suggestion of force—the note of a bird, the aspect of entrails, a mere change of feeling or shivering may be ominous. Thirdly, omens may often be remote from the event in time. Fourthly, omens do not generally indicate any particular event, but only success or failure in some enterprise (not the 'how' of it), happiness or misfortune. By such special characters certain noticeable antecedents stand apart from others as merely signs of the future; and the looseness of their connexion with the events in which men are concerned, facilitates their explanation by Animism (when Animism arises), as sent by spirits or gods. Magically, if a ground is ever sought for the connexion of omen and event, it may perhaps be found in the idea that both are involved in some current of invisible power: thus Seneca (a very late authority!) explains omens, as connected with the event in the same movement of Fate.

(*e*) The use of charms, rites and spells, having been established in one department—say hunting—may be extended to others by analogy; and in many cases they are confirmed there in the same way, namely, by doing one's best. Having obtained the charms and learnt the rites and spells, one still makes war or love as cunningly as one can, cultivates one's garden, drives a bargain, and so forth. But a time comes when Magic is transferred to practices that are purely dramatic; where a spear is 'sung' and waved but not hurled, or, if hurled, is not supposed to strike; invisible power issues from it and makes an invisible wound. As such practices have no effect, how can belief in them be established?

They give an imaginary satisfaction, which must suffice to initiate such pantomime again and again. Amongst ourselves, many people are apt to dramatize every situation of their lives; to act in imagination their loves, their revenges, their opportunities of self-display, and to derive satisfaction from such imaginations even to the weakening of their will—satisfaction without effort and without danger. But although this impulse may initiate pantomimic magic, it can hardly maintain it in the absence of any deeper satisfaction. The belief in its efficacy, again, once established, the effect of suggestion upon the victim (who by some means is made acquainted with what has been done) may have consequences that seem to verify the rites; but this can only happen when belief in the efficacy of such practices already prevails. The power of suggestion depends upon the belief; it cannot create the belief. We must fall back upon coincidence. If, indeed, immediate and complete coincidence were requisite, if when one practised on an enemy's life, nothing less than his speedy death would do, coincidences might be too scarce to give the requisite confirmation. But if some less injury will be acceptable, and if it need not follow immediately; if an interval of not merely two or three days, but two or three months will bring the event within the limits of satisfaction, and if the degree of injury may vary from death to a bad fall, or some failure in hunting on the part of the victim, or a quarrel with his wife; if even (as often happens) a misfortune to any one of his family may suffice; the necessary coincidences will be tolerably frequent. Finally, there must in fact be many disappointments; but these count for little, because the particular practice is supported by the general belief in Magic; because men desire to believe, and are afraid to disbelieve; because failures are explained by some error in performing the rites, or by the counteraction of superior Magic.

(*f*) The principles of mimetic and sympathetic Magic—that to operate upon a likeness, or by analogy, influences the person or process imitated, and that a part or appurtenance may be substituted for the whole in any magical undertaking—needless in the primitive devices of charm, rite and spell, are characteristic of the development of the art. The usual accounts of these principles—understood, of course, to be latent forms of inference; tacitly assumed, not formulated—are to my mind very unconvincing, though, to avoid further discussion, I adopted them in my paper on "The Conditions of Belief in Immature Minds"[1]. Savages are supposed not to distinguish similarity from identity, part from whole. Yet it is certain that in their work-a-day life they do make these distinctions, and that otherwise they could not get on at all. If, then, in certain cases and in Magic (which is all that concerns us now) they act or speak as if unable to make these distinctions, it must be an acquired incapacity; just as in some cases they suffer from an acquired incapacity to recognise that their beliefs are contradicted by experience; that is to say, some fixed idea and dissociation prevents them from seeing the facts; or sometimes it may merely be that customary forms of speech prevent the expression of distinctions that really exist among their ideas.

Supposing savages to have acquired such a habit of thought as to treat similars as, in Magic, identical; so that an image of a man may be substituted for him, and stabbed or burnt to his injury; it may have originated in more ways than one. If there is an impulse in the human mind to insure an event by reinstating all the circumstances formerly observed to precede it—in default of any one of them, whatever is most like it may be substituted. Dr Haddon tells us that in the western islands of Torres Straits[2] the *kuman* vine breaks up in dry weather, and the segments look like human bones; hence they are employed in Magic. Experience abounds with analogies to justify such a 'substitution of similars'; and what limits can be set to it; what degree of difference invalidates the substitution?

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[1] This *Journal*, VI. 315.

[2] *Reports of the Cambridge Expedition to Torres Straits*, V. 325.

Again, in sympathetic Magic, where a part is substituted for the whole, this may sometimes be done as the best means of completing the conditions (constituting a cause) when the whole thing or person (that should enter into it) cannot be obtained. But where a part of a man is injured in the belief that thereby the whole suffers, it is noticeable that the favourite parts by which to injure a man, hair-clippings, and nail-parings, are insensitive before their separation; injury is not felt in them; how, then, can it be felt in them when they have been separated? The confusion of ideas may have been arrived at thus: at first, the parts were not supposed to be substitutes for the man; they were merely means of identifying him; they fixed the wizard's attention, intensified his imagination, and directed the spell or curse (with which such rites are nearly always accompanied) whom to strike. It was the curse that slew: an invisible missile that inflicted an invisible wound. But the man being absent, and the hair, footprint, or what not, being the immediate object of attention and injury and revenge, it became a substitute for him; the curse was then less important, and might even be omitted[1].

Why may not the image of a man be also used at first as a means of fixing the attention, intensifying the imagination, and directing the spell, and have then become a substitute, the injury of which was equivalent to injury of the man? When some appurtenance of the man, or his image, is thus conceived as a substitute, the injuring of it gives a keener satisfaction of revenge than it could do merely as a memento of him. And it becomes intelligible that a stick or stone that does not resemble the enemy, but is merely conceived to be a substitute for him, may be employed in such rites.

In my former paper on savage belief, I adopted the view that images may be substituted for the enemy by reasoning that, as a shadow or reflexion of a man implies his presence, so does any likeness of him. But—without repudiating that view as one possible source of the illusion—I must point out that the presence of the enemy is not required for the purpose of the rite: the belief required by analogy is that to injure his reflexion (say, by breaking it with a stone), or his shadow

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[1] The tendency of that which is directly dealt with to take the place of that which is absent and merely thought of, is recognised. It may be illustrated by the way in which, among the Kayans, the omen-hawk becomes an object of reverence, instead of the gods whose messenger he is supposed to be. *Pagan Tribes of Borneo*, II. 9.

(say, by stepping upon it), is to injure him. That to step on a man's shadow injures him is a very common belief; but is it original; or may it not be derived from a prior belief in the substitutability of an image; or, again, from an animistic belief in the identity of shadow or reflexion with a man's soul?

Many rites and observances seem to depend upon the notion of a favourable or unfavourable current of invisible power, which may be taken advantage of, or influenced, as to hunting, rain-making, fertilisation, etc. It is good to plant crops or to begin any undertaking when the moon is waxing or the tide is rising; for these events show that the set of the current is favourable to increase or prosperity. Again, one may incite the current, or strengthen it, by making a beginning on a small scale, as in bringing on rain by spilling water, or leaping to help the crops to grow. To instigate or assist in this way the ongoings of nature is not the same thing as to cause the event by imitation: a rain-wizard does not pretend to make rain in the dry season; the times of ploughing, sowing, reaping (whatever rites may accompany them) are not decided by Magic. Much pantomimic Magic may be best understood as attempting to set up such currents of causation, rather than as directly causative. Since cases of cause and effect are often repeated, a pantomimic murder, or a hunting-dance, may be considered as setting an example, which Nature is expected to follow. The Kai of German New Guinea held that if a man, by falling on a stump of bamboo in the path, wounds himself to death, it is because a sorcerer, having obtained something infected with his victim's soul-stuff, has spread it over a pile stuck in the ground, and pretended to wound himself upon it, and to groan with pain[1]. The belief implies that such practices are in vogue; and I suggest that they rest upon the assumption that what happens once will happen again; and that who shall repeat the disaster is determined by the presence among the conditions of something belonging to the victim in default of himself.

(*g*) The rise of the wizard as a professional authority introduces many changes into magical practice. He sometimes experiments in new rites and spells, or in new versions of the old ones; and he sometimes decides arbitrarily, as in imposing taboos on food; and, generally, he developes the art in the direction of his professional interests, learning to act, conjure, suggest, hypnotize, and providing excuses for failure. How far arbitrary actions enter into rites it is impossible to say, until someone shall discover (as may be possible) marks by which to distinguish them. Meanwhile, there may be many rites, or ritual elements that cannot be explained on any principle as Magic, because in fact they are arbitrary: still such things must usually be an imitation of Magic. Prof. Leuba[1] suggests, probably enough, that rites may sometimes be invented to relieve excitement, such as the dancing of women when the men are at war. If I remember rightly, such was the practice of Araucanian women; and as they danced they swept the dust away with their fans, and sang: "As we sweep the dust away, so may our husbands rout the enemy." If this was arbitrarily invented, it was by analogy with much mimetic Magic; they set an example of what should happen, and confirmed it with a spell.

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[1] Quoted by Frazer: *Belief in Immortality*, p. 268.

(*h*) As the power of continuous reflexion, and the constructive faculty (which may have to be first trained in mechanical construction) grow, and writing comes into use, systems of Magic are produced, such as Cheiromancy and Astrology; but they are always mixed with Animism.

§ 10. THE DISSOLUTION OF MAGIC.

In spite of the human mind's strong proclivity to Magic, the art, as culture increases, gradually loses its predominance. As positive methods in war, building, commerce, are learnt and practised, the magical accompaniments of such undertakings, without being disused, become relatively less important—what we call 'survivals.' The great systems of Cheiromancy, Oneiromancy, Astrology, without being forgotten, become confined to what may be euphemistically termed 'select circles.' The spells, dances, plays are broken up or abbreviated. With the growth of Animism, magical observances are interpreted according to the new belief, no longer as setting occult forces to work, but as requiring the intervention of a spirit or god. They become symbolic. Ritual now does nothing of itself, but is a sign of what the god does or is desired to do. Yet Magic sometimes has its revenge upon Animism: enchaining by occult uniformities the god himself. Even in its own development, however, Magic contains some conditions of its own decline. Custom, whilst it maintains a practice, dispenses with its meaning, and slurs or corrupts the expression of it. There is also a tendency to the attenuation of rites, on the principle (unconscious, of course) that the sign of a sign is a sign of the thing signified; whereby a meaning is disguised for secrecy or even for politeness. Prof. Westermarck has shown how in

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[1] *The Psychological Study of Religion*, p. 165.

Morocco the full rite to avert the evil-eye is to thrust forward the hand with the fingers outspread, and to say—"Five in your eye." But as this is too insulting for common use, you may instead casually mention the number five; or if even that is too plain, you may bring in the word Thursday, which happens to be the fifth day of the week. It is obvious that in this process there is great risk of forgetting the original meaning of the spell; and when this happens we have complete retrogradation; in which condition are the current superstitions about 'thirteen,' 'Friday,' 'spilling the salt,' 'walking under a ladder'; for hardly a soul knows what they mean.

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SOME NEW REASONING TESTS SUITABLE FOR THE MENTAL EXAMINATION OF SCHOOL CHILDREN.

By W. H. WINCH.

I. The problem stated.
II. The nature of the tests.
III. The actual tests.
IV. The method of administering the tests.
V. The method of marking the tests.
VI. The improvement shown during the tests.
VII. The correlation between the results of these reasoning tests with school-standing as indicated by class or standard.
VIII. A method of determining the development of logical principles in school children.

I. The Problem stated.

It is well recognised that the so-called higher mental functions are not yet adequately represented by mental tests in current use among psychologists, although the importance of such tests has been urged. Of tests of higher functions, none seems of more value in mental diagnosis than tests of reasoning. The only tests which have attained very considerable vogue, and which at the same time are undoubtedly tests of reasoning, are Binet's. But his reasoning questions are open to some serious objections. For example, they savour too much of what the teachers call a 'catch'—a form of question which, happily, is fast disappearing from educational examinations. And further, the subject-matter of the questions is too advanced for young children.

For some years, therefore, I have been endeavouring to frame sets of reasoning tests of graded difficulty, which are suitable for children between 8 and 14 years of age, and can be answered by them collectively in writing. Such tests have been used in many schools of varying social class, of varying pedagogical proficiency, and of different sex. I do not propose to deal with the results of these tests on the large scale in this article; I wish only to endeavour to elucidate certain points of psychological interest in connexion with one set of them.

First, I hope to show, as indeed should be shown for all tests before they are generally applied, their very high reliability. Secondly, I hope to demonstrate, for all practical purposes, their non-improvability by practice. Thirdly, I hope to outline, from the consideration of the answers, a method by which the development of all logical and mathematical principles can be traced in the growing mind of the child.

II. THE NATURE OF THE TESTS.

I have stated that the tests required are such as are indubitably concerned with reasoning. But if they are to measure reasoning, as ordinarily understood, we must take care that the subject-matter of the question is well within the knowledge and experience of the children. Certain arithmetical problems are currently used for diagnostic purposes and satisfy this condition. But arithmetical problems are not satisfactory as diagnostic tests of reasoning for two reasons. First, a child may reason well but be bothered by the numerical relations involved in the question. Secondly, schools vary so much in the skill and thoroughness with which arithmetic is taught, that in solving arithmetical problems a child's behaviour is often more indicative of his school than of his own native ability. The psychologist must find other questions in which pedagogical influences are small or non-existent. He will find them, I suggest, in the principles of non-numerical reasoning, which we all make use of in daily life. But, of course, the questions must not be presented as questions of logical principles; these latter must be involved in interesting matters about Tommy and Mary, about shops and ships, about Sunday-school 'treats' and what boys and girls can do in their classes.

So framed, having regard always to the worldly experience of the children examined, the sets of tests admit of indefinite expansion into sets of equal difficulty. This is a valuable property for any test to possess, for we can vary the subject-matter, and yet keep the test constant in difficulty and constant in principle. Obviously, if this be so, we can obtain a real measure of the improvability or non-improvability for a particular set of functions of a rational kind. There are two theoretical difficulties in the way, but they can be overcome in practice.

First, no principle, logical or otherwise, is equally apparent in all the problems which embody it; and, secondly, the subject-matter of the problems may not be equally familiar to all the children tested. A careful framing of the problems, however, having in view both these pit-falls, will obviate them for all practical purposes, and, indeed, for many theoretical ones also. If these difficulties are really overcome, very high positive correlation will be found between the results of successive tests within the series. It will not be perfect, for there is a variability of the pupil as well as of the difficulty of the tests. Moreover, even in tests which show scarcely any practice-improvement on the whole, some, if not all, pupils do improve a little, and, as is usual in all tests, not altogether at the same rates. Hence, perfect correlation cannot be expected. But if the tests are carefully administered, all in precisely the same way and given at equally good pedagogical times, and after the same lessons, with serene and sympathetic non-interference on the part of the experimenter, the correlation-coefficients between successive equal tests of 'higher mental functions' will be very highly positive. The equality of the tests themselves will, of course, be primarily estimated by the person who frames them by means of general psychological analysis, largely supplemented by his actual knowledge of the minds of the children who are set to work them. The difficulty of the questions must be pitched so that all, or nearly all the pupils can do something, and no, or few pupils can do all, or nearly all the questions. Very high positive correlation between the successive tests of the series will form, as it were, the *a posteriori* justification of his *a priori* inventions.

Six equal sets of reasoning tests were used for this experiment, framed in accordance with the conditions laid down in this section. They will be given *in extenso,*—a necessary proceeding, since a good deal of preliminary work is required before getting them into the most satisfactory form, and it is hardly worth while for this to be done over again by other workers. Moreover, they are all of them of service practically in the selection of subnormals and have been used successfully for that purpose. With other reasoning tests sufficient to give a wide and comprehensive measure of rational functions, they can also be used to determine the disciplinary transfer values (if any), of certain rational school subjects like problematic arithmetic, grammar and demonstrative geometry.

III. The Actual Tests.

These tests may be conveniently referred to as Reasoning Tests Set G I, II, III, IV, V or VI, whilst the Problems comprising them are numbered 1, 2, 3, 4, 5, respectively, in each case. For example, G III 4 is the fourth problem of the third set of questions known as Set G.

Reasoning Tests Set G I.

1. Tommy had three silver coins, a sixpence, a shilling and a half-a-crown. The sixpence was brighter than the shilling, and the shilling was brighter than the half-a-crown. Was the sixpence brighter than the half-a-crown, or not so bright or just as bright, or can't you tell? You must give the reasons for your answer.
2. Some of the boys in Standard III. in a school you do not know got all their sums right last week. James was a boy in that class. Do you think he had all his sums right, or do you think he did not have all his sums right, or can't you tell? You must say why you think so.
3. A town called Burton is on the north side of a town called Sutton, and a town called Downham is on the north side of Burton. Is Downham on the north side of Sutton, or on the south of Sutton, or can't you tell? You must give reasons for your answer.
4. Tom had twice as many mistakes in dictation as Harry, and Jim had twice as many as Harry. Did Tom have more mistakes than Jim, or less than Jim, or the same number, or can't you tell? You must say why.
5. In a boys' school there were less than half of the boys who could play cricket, and less than half who could play football. Do you think there were any boys who could play both cricket and football, or do you not, or can't you tell? You must say why you think so.

Reasoning Tests Set G II.

1. Harry is heavier than John, and John is heavier than William. Is Harry heavier than William, or lighter than William, or the same weight, or can't you tell? You must give the reasons for your answer.
2. Some of the boys who go to West Street School wear blue caps. If you were to meet a boy in the street who goes to that school would you expect that he would have a blue cap, or would you expect that he would not have a blue cap, or can't you tell? You must say why you think so.
3. A toy-shop is on the left-hand side of a baker's shop, and a butcher's shop is on the left-hand side of the toy-shop. Is the butcher's shop on the left-hand side of the baker's shop, or on the right hand of the baker's shop, or can't you tell? You must give reasons for your answer.
4. James had half as many marbles as Tom, and Jack had half as many as Tom. Did James have more marbles than Jack, or less than Jack, or the same number as Jack, or can't you tell? You must say why.
5. Less than half the boys in a class had all their sums right, and less than half had no mistakes in dictation. Do you think there were any boys in the class who had all their sums right and also no mistakes in dictation, or do you not, or can't you tell? You must say why you think so.

Reasoning Tests Set G III.

1. Mary is shorter than Jane and Jane is shorter than Kate. Is Mary shorter than Kate, or taller than Kate, or the same height, or can't you tell? You must give the reasons for your answer.

2. Some of the grocers' shops in Mount Street are painted blue. If you went to a grocer's shop in Mount Street, would you expect to find it painted blue, or would you not, or can't you tell? You must say why you think so.

3. There are three ships on the sea, the Bugle, the Firefly and the Wasp. The Bugle is on the east side of the Firefly, and the Firefly is on the east side of the Wasp. Is the Bugle on the east side of the Wasp, or on the west side of the Wasp, or can't you tell? You must give reasons for your answer.

4. Jack had three times as many sums right as William, and James had three times as many sums right as William. Did Jack have more sums right than James or less sums right than James, or the same number as James, or can't you tell? You must say why.

5. Less than half the boys in a class were very good spellers, and less than half were very good at writing. Do you think there were any boys in the class who were very good at both writing and spelling, or do you not, or can't you tell? You must say why you think so.

Reasoning Tests Set G IV.

1. James can run faster than Tom and Tom can run faster than Harry. Can James run faster than Harry, or not so fast, or just as fast, or can't you tell? You must give the reasons for your answer.

2. Some of the boys in Standard IV. are very good at woodwork. James is in Standard IV. Do you think he is very good at woodwork, or do you not, or can't you tell? You must say why you think so.

3. Three girls are standing in a row. Jane is on the left hand side of Mary, and Mary is on the left hand side of Kate. Is Jane on the left hand side of Kate, or on the right hand side of Kate, or can't you tell? You must give reasons for your answer.

4. Smith Street was four times as long as Sun Street, and Warner Street was four times as long as Sun Street. Was Smith Street longer than Warner Street, or shorter than Warner Street, or the same length, or can't you tell? You must say why.

5. In a class of girls less than half were very good at drawing, and less than half were very good at needlework. Do you think there were any girls who were very good at both needlework and drawing, or do you not, or can't you tell? You must say why you think so.

Reasoning Tests Set G V.

1. Harry can write better than Jim, and Jim can write better than Tom. Can Harry write better than Tom, or not so well, or just as well as Tom, or can't you tell? You must give the reasons for your answer.

2. Some of the little boys in Standard I. can draw very nicely. If you knew that a little boy was in that class, would you think that he could draw very nicely, or would you not, or can't you tell? You must say why you think so.

3. There are three shops in a street, a grocer's, a baker's and a butcher's. The baker's shop is on the west side of the butcher's, and the grocer's is on the west side of the baker's. Is the grocer's on the west side of the butcher's, or on the east side of the butcher's, or can't you tell? You must give reasons for your answer.

4. James could run five times as fast as little Johnny and Harry could run five times as fast as Johnny. Could James run faster than Harry, or just as fast, or not so fast as Harry, or can't you tell? You must say why.

5. Less than half the boys in a school got prizes for reading, and less than half got prizes for drawing. Do you think there were any boys in the school who got prizes for both reading and drawing, or do you not, or can't you tell? You must say why you think so.

Reasoning Tests Set G VI.

1. There are three countries, Harland, Norland and Firland. Harland is larger than Norland, and Norland is larger than Firland. Is Harland larger than Firland or not so large, or just the same size, or can't you tell? You must give the reasons for your answer.

2. Some of the 'buses which go into the country have little flags on them. If you saw a 'bus going into the country, would you expect it to have a little flag or would you not, or can't you tell? You must give reasons for your answer.

3. There are three books, a red one, a blue one and a green one. The blue one is underneath the green one, and the green one is underneath the red one. Is the blue one underneath the red one, or on top of the red one, or can't you tell? You must give reasons for your answer.

4. James has been away from school six times as much as Harry, and Tom has been away six times as much as Harry. Has James been away more than Tom, or less than Tom, or just as much, or can't you tell? You must say why.

5. Less than half the girls in a school wore blue frocks, and less than half wore straw hats. Do you think there were any girls in the school who wore blue frocks as well as straw hats, or do you not, or can't you tell? You must say why you think so.

IV. THE METHOD OF ADMINISTERING THE TESTS.

The tests were done by *all* the eleven-year-old children in a municipal elementary school for boys, situated in a rather poor neighbourhood in the south-east of London. The school was a strongly disciplined and hard-working one, and the boys might, on the whole, be relied upon to give their full attention to the work even after the influence of novelty had passed away.

The Reasoning Tests formed part of a series consisting also of Tests in Simple and Complex Perception, in Rote and Substance Memory and in Productive Imagination. The exercises were worked according to a definite time-table, commencing on Wednesday, June 12, 1912, at 9.45 in the morning, and on the same day at 2.15 in the afternoon, six tests being done on the one day, three in the morning and three in the afternoon. Two days later (Friday, June 14), the second series of tests were worked, then on the next Wednesday a third series. The work continued thus on Wednesdays and Fridays, until each boy had done six tests of each kind, the concluding exercises being given on Friday, June 28.

Each morning's work began with a Simple Motor Test; it was immediately followed by a test in Substance Memory; and it concluded with a set of Reasoning Tests, which began at 10.40. The problems in Reasoning were written on large black-boards and were exposed one at a time. They were read slowly and carefully, one at a time, by the head-master, a man experienced in experimental work with children. Then, one by one, with no fixed time limit, the boys wrote their answers, the next problem not being exposed or read until each boy had done all that he could on the preceding problem.

The boys were delighted to work these exercises, for it is very rarely that they know that they cannot answer such questions rightly. They find something to say, even if irrelevant to the required answer, and are usually fairly satisfied with their efforts. Of course, no explanation or information respecting the tests was given throughout the experiment.

V. The Method of marking the Tests.

It would not be difficult for students of Logic to set out the solutions at once and mark by means of their own answers. But that method is not usually very satisfactory in Child Study; and in any case, it is a much more convincing method to consider the answers of the children first, and mark on the lines of their own developing thought. So I shall present a few specimens of the boys' answers, some good, some not so good, and show what the children make of the problems. I shall suggest a method of marking some individual cases before setting out a *system* of marking. I quote first one or two of the best papers.

The Reasonings of Edward W——, aged 11 years 5 months, Standard V.

First Set of Answers. Set G I.

1. If the sixpence was brighter than the shilling and the shilling was brighter than the half-crown then the sixpence was brighter than the half crown because if the sixpence was brighter than the shilling and shilling than the half crown then the sixpence was brighter than the half crown.

2. I cannot tell because it only says that some of the boys got their sums right, he might have got them right or he might not I cannot tell.

3. If Burton is on the north side of Sutton and Downham on the north side of Burton It must be north because if it was south it would say so.

4. Tom had the same as Jim because Tom had twice as many as Harry and Jim had twice as many as Harry so they were both the same.

5. I cannot tell because it says less than half the boys could play cricket and less than half the boys could play football there might have been some boys who could play both or there might not I cannot tell.

Edward's first answer is right, but he need not have repeated his premises in the second part of his answer. His second answer is quite correct, though it is longer than is really required. His third answer is marked wrong because his reason is given wrongly; it displays an undue confidence in the explicitness of logical problems. His fourth answer is correct; and his fifth answer is marked right, but does not carry the analysis far enough to obtain full marks. There might have been some who could play neither. Not both is an ambiguous expression; it includes the case in which some can play one game and not the other, and also the case in which some can play neither. For this section of his answer he obtains one mark, and for the possibility that some can play both he receives one mark; he also receives a mark for making his reason explicit. He therefore receives six marks as a total for his first paper; and in his next set of answers he obtains full marks. His paper is as follows:

Second Set of Answers. Edward W——. Set G II.

1. Harry is heavier William because if Harry heavier than John and John heavier than William then Harry heavier than William and John.

2. I can't tell because it only says that some of the who go to West Street School wear blue caps, he might have a blue cap or he might not I cant tell.

3. The butchers shop is on the left hand side of the bakers shop because If the toy shop is on the left hand side of the bakers and the butchers shop is on the left hand side of the toy shop then it is on the same side.

4. James as the same as Jack because they both had half as many as Tom.

5. I can't tell because it says less than half the boy got their sums right and less than half with their dictation right so there might have been some boys with both or their might have been some boys with one and there might have been some with neither.

In his second set of answers, Edward scores full marks, namely, eight. He has shortened his first answer, giving his reason once only; his second is still needlessly long; his third is quite right, the reason being correctly given on this occasion, and his fourth is more succinct than in his first paper. His last answer carries four marks, he has given all the possible cases and stated his reason correctly.

In his third set of answers Edward also scores full marks, as he does also in his fourth set. In the fifth set he makes his second answer more explicit; he says:

Fifth Set of Answers. Edward W——. Set G V.

2. I can't tell because it says that some boy are good drawers and some are not every one.

He retains this form of answer to problem 2 in his sixth and concluding set of answers. But he loses a mark in his answer to problem 5 in the fifth set, since he omits his reason, and he loses a mark for the answer to the corresponding problem to the sixth set, since he inaccurately quotes his premises as "some of the girls" instead of less than half.

Edward's papers are excellent; on three occasions he scores full marks—the only three occasions on which full marks are scored by any boy throughout the whole series of these exercises. Quite obviously, in this case, there is little room for improvement; though he does score two marks more on his second paper than on his first.

As a strong contrast, I present some of the answers of one of the weakest eleven-year-old boys in the school.

THE REASONINGS OF VICTOR S——, AGED 11 YEARS 8 MONTHS, STANDARD III.

First Set of Answers. Set G I.

1. Whey the sispence wasent briter at all they were all the same as briter as each other.
2. they all had them all rite because why they were write because they tired
3. Sutton is on the north side of Burton because sutton is on the north side of Burton
4. Jim had as much as Harry they all ad the same
5. they could play cricket better than football is hard to play cricket is best some can play football and some can play cricket

Victor's paper shows the confidence with which boys who cannot do them tackle these tests. They are interested in them and 'give themselves away' most cheerfully. I need hardly point out how important it is in psychological tests that this condition should be fulfilled.

This boy scores no marks either for this or for any of his succeeding sets of answers.

He denies the premises in his first answer; converts "some" into "all" in his second answer and backs his own misconception by an invented reason; in the third he gives what has been (somewhat unfairly) called a girl's reason; in the fourth, he gets the right solution, but cannot give a reason for it; and in the fifth, as might have been expected, he never advances beyond the premises, and these, indeed, not rightly stated.

He makes no advance throughout the whole series, though he continues to write with complete confidence; on the contrary, he falls away a little, for in his fourth and fifth set of answers, he gets even his *solution* wrong for the fourth problem, thus:

Fourth Set of Answers. Victor S——. Set G IV.

4. Smith St is longer than Warner St is longer than smith St

Victor repeats this form of answer in problem 4 of the fifth set, though he goes back to his first solution in the corresponding problem of the sixth set. Only in one answer does his confidence flag, for instance:

Sixth Set of Answers. Victor S——. Set G VI.

2. Yes, the flag on the bus, yes it has, I dont no.

I will give one more set of very inferior answers: one other boy, and only one other, scores no marks throughout the exercises, and his work, together with that of Victor S——, gives us, as it were, a lower limit for these eleven-year-old boys. Victor sometimes denies the premises given to him; the boy, whose work I now present, is of a somewhat higher type; he does not deny the premises, although sometimes getting a right solution; but gives, not the reason which forced him to his solution, but a suggestion of his own which might account for it.

THE REASONINGS OF HENRY W——, AGED 11 YEARS 6 MONTHS, STANDARD IV.

First Set of Answers. Set G I.

1. The sixpence was brighter than the half-crown because the sixpence might had bean new.
2. The boy in Standard III. did not get his sums right because he do not think over his sum
3. Bunton is on the north side because Sutton is on the south side of Bunton
4. Tom had the same as Jim because they might have been two good splesers (*spellers*)
5. Some boys could play cricket and football because they might had seen other boys play football.

Henry has written a neat and confident paper. I have already described his leading characteristic, by no means unusual in boys and others who are bad reasoners. In his third answer he omits Downham altogether; he places Burton correctly—one premise has given him that—and then gives a reason for it which is a consequence of the one premise. These characteristics of Henry's reasonings are manifest in all his papers. I quote, for example, his fifth set of answers:

Fifth Set of Answers. Henry W——. Set G V.

1. Harry can write better than Tom because Harry can write better Jim.
2. A little boy in Standard I can draw nicely because he ben learnt

3. The grocer's shop is on the west side of the butchers because the bakers is on the east side of the butchers.

4. James can run faster than Harry because James can run faster than little Johnny.

5. Some of the boys got prizes for reading and some drawing becaus some are good reading and some are not.

In his first answer, it is possible that Henry is on the road to stating his 'reason' correctly; in three preceding papers he has given up accounting for his solutions to this problem (which are correct, be it remembered) by a suggestion of his own, and, on this occasion, states one premise correctly. In his second answer he falls back upon his old method of guessing a solution, and supporting it by a hypothetical premise. The answer to problem 3 looks like either sheer perverseness or mere muddleheadedness,—the baker's shop is not on the east side of the butcher's, and if it were, that fact would not justify the conclusion that the grocer's is on the west side of the butcher's. But this is probably not the case, since Harry's answer to the corresponding problem in the second set is quite similar. He writes:

Second Set of Answers. Henry W——. Set G II.

2. The butcher's shop is on the right hand side of the bakers because the toy shop is on the left hand side bakers.

The understanding of these two difficult answers cannot be forced from within; but in very poorly developed children of this age, an implicit axiom of spatial reasoning is discoverable, which runs thus: 'Two things cannot be on the same side of the same thing. If one is on the left the other is on the right.' This is not merely a suggestion of my own: in other sets of Reasoning Tests, with open-minded children, I have had this 'reason' clearly stated. Of course, *side* means *position* to them, not *direction*. For such children, the question, considered purely as a problem in reasoning, is not a good one. The number of these children in the present research is, however, inconsiderable. In answer to the corresponding problem of the last set, he gives no reason. In his other answers to the third problem, Henry adheres to his first method of reasoning, though he makes an advance in correct placing. For instance:

Fourth Set of Answers. Henry W——. Set G IV.

3. Jane is on the left side of Kate becaus Kate is on right hand side of Jane

His remaining answers to the fifth set of problems call for no special comment: his work is remarkably uniform throughout, and his general method of dealing with problems in reasoning has already been indicated. He scores no marks whatever throughout the exercises.

But, of course, as in all mental tests, psychological interest is but little attached to those who completely fail, though the kind of error is significant, and is, perhaps, less attached to those who are wholly right, though such cases form an upper limit, as it were, and show us the methods of thought among able children. The really interesting cases to the psychologist are those in which error gradually gives place to accuracy. This is difficult to trace in such tests as these if we confine ourselves to the answers of the same boy throughout; for, as I have indicated, there is little improvement after the second exercise. One or two boys, however, do show a tangible advance. The papers of Frederic W—— are interesting in this connexion.

THE REASONINGS OF FREDERIC W——, AGED NEARLY 12 YEARS (he was 12 at the end of the month in which the exercises were done), STANDARD VI.

First Set of Answers. Set G I.

1. If the sixpence is brighter than the shilling, and the shilling brighter than the half-crown the sixpence is the brightest.
2. He did not get all his sums right because only some of the boys got them right.
3. Downham is on the north side of Sutton, because Burton is on the north side of Sutton.
4. Tom and Jim have the same mistakes because they both have twice Harry.
5. Half the boys can play cricket and football because a quarter play cricket and the other quarter play football.

Frederic's first answer is quite correct and clearly stated. His second is wrong, for the boy might have been among the 'some' who got the sums right. His third answer is marked wrong because he gives only one premise of his conclusion. His fourth answer is correct, if we allow "the same mistakes" to be equivalent to 'the same number of mistakes,' which we may fairly do, considering the form of the question. In the fifth answer, Frederic assumes that less than half means a quarter, and assumes that it cannot be the same quarter, or overlapping quarters, which play cricket and football respectively. He scores two marks as a total, for his first set of answers.

He makes no advance in his second set of answers; but in his third paper, whilst giving identical answers to all the corresponding problems 1, 2, 3 and 4, he advances by two marks in the last problem, thus:

Third Set of Answers. Frederic W——. Set G III.

5. I cant tell, because less than half are good spellers, and less than half very good writers.

This answer scores two marks, one for the inability to tell and one for the statement of his reason.

In his next set of answers there is a still further advance. This time he answers problem 2 correctly, thus:

Fourth Set of Answers. Frederic W——. Set G IV.

2. I cant tell, because only some of the boys are good at woodwork.

For the first time he realises that the statement involving 'some' does not allow us to infer anything certain about any particular boy.

In the third answer he quotes both premises on this occasion, but places Jane on the *right* hand side of Kate; this, of course, is wrong; his other answers are identical with those to the corresponding problems in his previous work.

In his fifth set there is a further advance, though a very small one, not sufficient to carry a mark: he has become undecided in the 'position' question (problem 3). He quotes the premises correctly as his reasons, and says, "I cant tell." His other answers are identical with those of the preceding sets.

In his sixth and final set of answers he reverts to his old and inaccurate solution for problem 2, thus:

Sixth Set of Answers. Frederic W——. Set G VI.

2. It would not have a little flag because some buses had a little flag.

But his answer to the third problem is for the first time correct, as follows:

Sixth Set of Answers. Frederic W——. Set G VI.

3. Red one is on top of the blue one because blue underneath green and green underneath red.

Thus, for his sixth paper, he loses one mark formerly gained, and gains one mark formerly lost, his total of five remaining the same. Had more than one or two boys shown the same advance as Frederic the general conclusion of non-improvability could hardly have been maintained. One boy goes steadily down from six marks to five and from five to three; thus:

THE REASONINGS OF LEONARD N——, AGED 11 YEARS 10 MONTHS, STANDARD VI.

First Set of Answers. Set G I.

1. The sixpence is brighter than the half crown because the sixpence is brighter than the shilling and the shilling brighter than the hf crown so the sixpence must be brighter than the hf crown.

2. I could not tell until I had asked him because it say's *some* of the boys got all their sums right.

3. Downham is on the north side of Sutton because Burton is on the north side of Sutton and Downham is on the north side of Burton so Downham must be on the north side of Sutton.

4. Tom had the same number of mistakes as Jim because Tom had twice Harry's mistakes and Jim had twice Harrys mistakes they both had twice Harry's ∴ they both had the same number

5. No boys could play both, because Less than half could play cricket and less than half who could play football, so no boys could play both.

Leonard's answers to problems 1, 2, 3 and 4 call for little comment; they are correctly solved, clearly reasoned and well expressed linguistically. His fifth answer is worth two marks. He is, so to speak, starting at opposite ends of the school; a procedure, which, of course, gives one of the three possible cases, each of which carries a mark, if pointed out. His statement of the reason also carries one mark. How, then, is it that Leonard, who reasons extremely well for an eleven-year-old boy, falls off subsequently from six marks to five and then to three?

In his second paper he leaves out the redundancy about 'asking' in the second problem, and shortens his answer to problem 4; otherwise his reasonings and even his phrasings are identical with those of his first paper.

In his third paper he again obtains six marks, though he has arrived at a new stage of understanding about problem 5. He now says:

Third Set of Answers. Leonard N——. Set G III.

5. I cant tell, because less than half were good spellers and less than half good at writing so you cant tell.

He receives one mark for stating that he is undecided and one for correctly stating his reason, and to this form of answer he strictly adheres throughout the rest of his answers to problem 5. I am doubtful whether this is superior or inferior to his previous ones: in any case it carries the same mark. His other answers to the problems of Set 3 are identical with those to the corresponding problems in his previous papers. In paper number 4 he loses two marks on his previous record, as he does also in papers 5 and 6, and precisely in the same way. He gives one premise only in his answers to the first and third problems of Sets 4, 5 and 6, thus:

Fourth Set of Answers. Leonard N——. Set G IV.

1. James can run faster than Harry because Tom can run faster than Harry.

3. Jane is on the left hand side of Kate because Jane is on the left hand side of Mary.

The answers to problems 1 and 3, in Sets 5 and 6, are identical with those given above. It happens sometimes that the excellence of later exercises in a series worked by schoolboys may decline owing to the influence of boredom, rather than to a real decline in power. This certainly seems to be the case with this boy; for his earlier answers are expressed with a fullness and point which are lacking in the later ones. If it be true that the exception proves the rule, I may be allowed to quote this case because it is so exceptional.

I have illustrated this paper by means of sets of answers which are excellent throughout, by others which are bad throughout, by others which show considerable improvement, and by others which show some deterioration. In further illustration I will quote the work of one more boy; on this occasion one who is more typical, since his marks fluctuate slightly.

THE REASONINGS OF EDWARD G——, AGED 11 YEARS 4 MONTHS, STANDARD VI.

First Set of Answers. Set G I.

1. The sixpence is brighter than the halfcrown. Because the sixpence is brighter than the shilling and the shilling is brighter than the half crown so it is brighter.
2. I dont know whether he got his sums because he might not have been one of the boys.
3. Downham is on the north side of Sutton because Burton is north of Sutton and Downham is on the north of Burton.
4. They both had the same because they both had twice Harry.
5. There were none who could play both because there was less than half in each game.

Edward G. scores five marks for his first paper. He is obviously correct in problem 1; but is not explicit enough to gain a mark for his second answer. It is not sufficient to say he *means* right, for if we allow that "*one of the boys*" is really a reference to the 'some' boys given and not to the whole class, Edward ought also to say that he might have been, as well as that he might not have been.

His third answer is good and clear: his fourth good and very tersely put: and his fifth answer gains two marks, though he has given his one possible case too definitely. His second paper, with the exception of the second answer, is identical with his first. His second answer now runs:

Second Set of Answers. Edward G——. Set G II.

2. I cant tell because he might be one of these boys and he might not be

This answer receives a mark since it seems clear that he correctly reasoned out the case from the meaning of 'some.' Edward, therefore, receives six marks for his second set of answers.

His third paper is identical with his second, except that in his second answer he falls back on the more imperfect form of his first solution to the second problem; and his total mark therefore falls to five for this set.

In his fourth paper he returns to the form of answer to the second problem which he gave in his second paper, and scores a mark for it. His other answers are identical, so that his mark moves back to six.

In his fifth paper, he retains his better form of answer to problem 2: but breaks down in problem 3, the 'position' question. He sees, however, for the first time, that his fifth answer has hitherto been too definite.

Fifth Set of Answers. Edward G——. Set G V.

3. It is on the west. because the bakers is in the middle the grocers is on the east, and the butchers on the west.
5. I cant tell. Because there were less than half in each.

A glance at the question in problem 3 of Set V shows that he has given his premises incorrectly or reasoned incorrectly from those he has accepted. His fifth answer gains two marks, one for his apprehension that the problem is indeterminate, and one for correctly stating his premise; he does not now exclude other cases as he did in his first, second, third, and fourth papers. His total mark remains at six.

In his sixth and concluding paper, he regains his mastery over the position question, but otherwise repeats his answers to the fifth set of problems; for the first time he scores seven marks.

The reader who, with the questions before him, has followed the boy's work which has been cited, will by now have little difficulty in marking these exercises.

The answer to the first problem is marked correct if the correct solution is given together with the appropriate statement of the two premises. The axiom of reasoning may be rather clumsily stated in the form—'that which is more than the greater is more than the less'; but, of course, as with all of us, boys can reason on a principle long before they can state it in 'formal' terms. I am using the word 'formal,' because, whatever we may think of the body of doctrine known as Formal Logic, there are certainly forms of reasoning that may be illustrated by many and various concrete examples which, indeed, in a logical sense are dependent upon these 'forms'; though psychologically the 'forms' themselves emerge from a consideration of the instances which embody them. That a working, *i.e.*, an applicable comprehension, of these forms does arise, which is of practical service, is clear, from children's answers not only to logical problems, but also to those concerning more definitely quantitative reasonings, as shown in their work in problematic arithmetic.

The correct answer to the second problem requires the child to apprehend and clearly state that on the basis of 'some' he cannot infer with certainty to an individual case. This the child sees long before he realises that if the 'some' is large enough, he *should* so infer,—a consideration which is at the basis of most modern scientific work and is involved, indeed, in all arguments from degrees of correlation.

To gain a mark for the third problem the boy must correctly place the things given, and state both premises of his judgment. He is not required to give the 'axiom' on which it is based which can, indeed, only be stated very clumsily by any of us.

In the fourth problem, the correct solution must be given, together with the reasoning on which it depends. We do not require the child to say that the same multiples or fractions of the same thing are equal to one another, though I should quite expect clever adolescent boys to assert this.

The fifth problem is much the most difficult. Readers of Logic will remember Euler's diagrams with their inclusion and exclusion of classes; and I can well believe that similar diagrams, applied to this problem, would be most illuminating to the pupil; but I do not now propose to discuss the pedagogical problems of Logic. Suffice it to say that if a boy says he cannot tell, and correctly states his premises, he receives two marks. Some boys have, however, received more than two marks. They have analysed the problem into its constituent cases, or into some of its constituent cases. For each case cited the boy has obtained a mark, provided that he has also cited his premises correctly, for which he receives an additional mark. If, in thought, we start at opposite ends of the school or class with the 'less than half,' we have none who is common to the two groups; if we start from the same end, as it were, we may have complete identity between the two groups; or we must at least have some overlapping; and if we start with the two groups of less than half in intermediate positions, we may have overlapping or we may not. The full answer therefore requires either, neither, or both, as the solution. Of course, in this answer, as in the other problems, the boy is required to state his reason. A complete answer to this problem carries therefore four marks, obtained, I think, less than a dozen times throughout the whole series of exercises.

Further researches on children's reasonings may show that it is desirable to adopt a more refined system of marking than that here adopted; but it is obviously exact enough for purposes of general mental diagnosis, and I believe that it may be exact enough for tracing the evolution of logical 'forms' in the minds of school-children; but on this latter question, I propose to say something in another section.

TABLE I. *Showing Test by Test, and Child by Child, the Marks for each Set of Reasoning Problems worked by all the eleven-year-old boys of School "O. K."*

School Standard	Child's Initials	Age		1st Ex. 12.6.12 Wed.	2nd Ex. 14.6.12 Friday	3rd Ex. 19.6.12 Wed.	4th Ex. 21.6.12 Friday	5th Ex. 26.6.12 Wed.	6th Ex. 28.6.12 Friday	Totals for 6 Tests
		Yrs	Mths							
III	V. S.	11	8	0	0	0	0	0	0	0
IV	W. P.	11	0	4	5	5	4	5	4	27
,,	T. S.	11	7	1	2	3	5	3	3	17
,,	H. W.	11	6	0	0	0	0	0	0	0
V	A. B.	11	7	2	4	6	7	6	6	31
,,	S. B.	11	11	7	6	6	7	7	6	39
,,	W. H.	11	8	6	6	6	6	7	6	37
,,	C. J.	11	6	3	4	5	4	5	4	25
,,	A. K.	11	4	5	6	6	7	7	6	37
,,	J. M.	11	11	3	6	5	5	6	6	31
,,	H. P.	11	10	6	4	3	4	4	3	24
,,	T. S.	11	5	3	5	6	5	6	6	31
,,	H. W.	11	11	4	3	3	4	4	4	22
,,	E. W.	11	5	6	8	8	8	7	7	44
,,	R. M.	11	9	4	3	4	5	5	5	26
VI	W. B.	11	11	2	4	3	3	2	3	17
,,	A. D.	11	3	4	4	5	3	5	5	26
,,	G. F.	11	6	2	3	4	5	4	6	24
,,	C. F.	11	2	1	1	1	1	1	2	7
,,	E. G.	11	4	5	6	5	6	6	7	35
,,	J. H.	11	6	4	4	3	4	3	3	21
,,	W. H.	11	1	2	4	5	5	4	4	24
,,	W. M^{c}C.	11	6	4	5	4	5	5	5	28
,,	W. N.	11	2	3	5	5	4	5	5	27
,,	L. N.	11	10	6	6	5	4	4	4	29
,,	A. P.	11	5	5	6	4	4	4	4	27
,,	W. P.	11	6	7	7	5	5	5	5	34
,,	R. R.	11	5	1	2	1	2	2	2	10
,,	H. S.	11	9	2	1	1	1	1	1	7
,,	L. S.	11	11	2	3	2	2	3	2	14
,,	W. S.	11	2	4	5	4	6	7	6	32
,,	W. W.	11	5	3	5	6	6	5	5	30
,,	F. W.	11	3	2	2	4	5	5	5	23
,,	V. W.	11	5	2	3	3	4	2	2	16
VII	E. H.	11	6	5	4	6	6	5	6	32
,,	C. K.	11	11	4	4	4	4	4	5	25
,,	J. N.	12	0	1	4	4	4	6	6	25
,,	G. B.	11	10	2	5	4	4	5	6	26
,,	W. H.	11	4	7	6	6	6	7	7	39
,,	R. J.	11	8	5	6	6	5	6	6	34
,,	E. M.	11	10	6	6	6	6	6	5	35
	Totals			145	173	172	181	184	183	1038
	Averages	11	6·6	3·5	4·2	4·2	4·4	4·5	4·5	25·3

VI. THE IMPROVEMENT SHOWN DURING THE TESTS.

One of the reasons for presenting these results is the need felt at the present time for Mental Tests which deal with what are known as the 'higher' mental functions; and, at the same time, give a series of steady results, not much, if at all, affected by improvement by practice.

As the Tests are new, I propose to give a table showing the individual results, exercise by exercise. The marks given are the total marks, test by test, for each set of problems; the maximum mark, it will be remembered, being eight.

A careful scrutiny of the individual results shows that there is little improvement after the second exercise. But as this is an important issue, I have worked out the statistical checks as follow. The correlation-coefficient is calculated on Pearson's r formula and the 'probable error' of the differences between the successive means by the formula $\text{P.E.} = \cdot 67\sqrt{\frac{\sigma_1^2 + \sigma_2^2 - 2r\sigma_1\sigma_2}{n}}$, in which r is the correlation-coefficient, n is the number of cases, and σ_1, σ_2 are the standard deviations of the successive pairs of averages.

TABLE II. *Showing the Correlation-coefficients, the Differences between the Means, and the Probable Errors of those Differences.*

Between Exercises	Correlation-Coefficients	Differences between the Means	Probable Errors of the Differences
1 and 2	+ ·80	·7	·12
2 ,, 3	+ ·87	·0	·09
3 ,, 4	+ ·88	·2	·09
4 ,, 5	+ ·90	·1	·09
5 ,, 6	+ ·94	·0	·07

The correlation-coefficients between the Tests are uniformly very high,—a vital consideration when answering the question as to whether a mental test works satisfactorily. The results are constant throughout, which, in a test of this kind, is some guarantee that the various sets of tests are of equal difficulty; and after the first increase from exercise 1 to exercise 2, there is not enough improvement shown to spoil our tests as steady measures of a particular group of functions. I do not wish to be understood as asserting that *no* improvement takes place during a long series; there is an apparent increase from exercise 3 to exercise 6, but the probable error is considerable in relation to the difference between the means, and considered as successive tests, notwithstanding the high correlation-coefficients, the differences between the means, after the first leap up, are always less than three times their probable errors. Probably, in all work with mental tests where great exactitude is required the first test of a series should be neglected.

VII. THE CORRELATION BETWEEN THE RESULTS OF THESE REASONING TESTS WITH SCHOOL-STANDING AS INDICATED BY CLASS OR STANDARD.

I give first a table showing, standard by standard, the results of the six sets of tests.

TABLE III. *Showing the relation between the school-grading of the children and the marks for* 6 *sets of problems.* (Reasoning Tests. Set G.)

School Standard	No. of boys	Av. age Yrs.	Mths.	Average Marks for Exercises 1st	2nd	3rd	4th	5th	6th	Totals
III	1	11	8	0·0	0·0	0·0	0·0	0·0	0·0	0·0
IV	3	11	4	1·7	2·3	2·7	3·0	2·7	2·3	14·7
V	11	11	8	4·5	5·0	5·3	5·6	5·8	5·4	31·5
VI	19	11	5	3·2	4·0	3·7	3·9	3·8	4·0	22·7
VII	7	11	7	4·3	5·0	5·1	5·0	5·6	5·9	30·9

This is an unusual result; it is almost invariably found that standard by standard there is a steady rise. I say this on the basis of the results of many sets of reasoning tests worked throughout whole schools. Why then do I quote this contrary result? Briefly, because it illustrates the need for inquiry into any special mental bent in the teachers of the classes. The teacher of Standard V is a young man with special interest in, and knowledge of, logical problems; he has attended University classes in Logic and given deep consideration to logical questions. His is certainly a very rare case among elementary teachers, so that we need not despair of the value of these tests in mental diagnosis generally. Here, however, is a special case in which the influence of the teacher, exercised through the ordinary subjects of instruction (logic, of course, is not taught), has placed his boys higher than the first class in the school (Standard VII)[1]. Of course, the assertion of a pedagogical factor is an easy way out of a difficulty; but it is an assertion which has experimental ground. For it may be remembered that these Reasoning Tests were part of a series of tests embracing Simple and Complex Motor Perception, Rote and

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[1] I was, of course, not content without further and more careful determination of the pedagogical proficiency of the children, but I do not propose to give the correlation-coefficients worked out from the individual cases between pedagogical proficiency and natural ability to reason, as indicated by these tests; it would be misleading owing to the pedagogical factor I have indicated.

Substance Memory and Productive Imagination, and in all of these the class taught by this teacher takes its proper place in an ascending order of proficiency, for this school is a well-organized one.

VIII. A METHOD OF DETERMINING THE DEVELOPMENT OF LOGICAL PRINCIPLES IN SCHOOL CHILDREN.

From the instances of childrens' reasonings already given, the reader will have formed some notions as to the development of certain logical principles in so far as they are illustrated in the foregoing sets of tests. For the moment, however, I wish rather to indicate how a system of tests, constructed and arranged as above, may enable us to trace the natural growth of any principle of reasoning, logical or mathematical. The framer of such tests must have in mind some logical principle whose development he wishes to trace. Then, problems having reference to the actual experience of the child must be constructed exemplifying the principle, and they must be problems of equal difficulty. They should be administered at regular intervals, and, if possible, after the same school lessons, to children of differing ability and of different ages. And they must, when the tests have been shown to be reliable, be administered on a scale of considerable magnitude. Work on the small scale like that of the present research cannot be regarded as more than suggestive, but it is useful in indicating directions of development. Research on a large scale in which whole schools are dealt with is imperative before we can demonstrate general lines of development. I have worked with sets of reasoning tests on such a scale, but not with this particular set. Consequently I make the suggestions which follow as to the development of the logical principles or 'forms' of reasoning involved in the above tests without claiming that I have demonstrated lines of general development. What I am concerned to press is the value of the method if developed and carried out on a larger scale. A few suggestions, not demonstrations, of the development of logical 'forms' follow:

i. *The Development of the Answers to Problem* 1.

A preliminary stage, of course, is that in which the premises have not been thought together: there is no solution at all. This stage is illustrated by the first answer of a boy, who is, on the whole, an able boy, and succeeds in the corresponding problem of all his other papers. It is interesting because it is a clear statement that the premises give no ground for a conclusion, thus being above mere confusion.

First Set of Answers. George B——. Set G I.

1. One cannot tell! The shilling was brighter than the half-crown, but we can't tell which was the brighter of the sixpence and half-crown.

Another preliminary stage is that in which thinking them together has distorted the premises themselves. The solution is wrong and the premises, if referred to at all, are distorted. Such a stage is probably indicated in the answer of Victor S——, aged 11 years 8 months, Standard III.

First Set of Answers. Victor S——. Set G I.

1. Whey the sispence wasent brighter at all they were all the same as briter as each other.

To this stage belongs also the following answer:

First Set of Answers. William H——. Set G I.

1. The half-crown was the brightest because the half-crown is worth more. I would be brighter with half a crown than with sixpence. You might change the half crown and get five bright sixpences in exchange.

The third stage is one in which the correct solution is arrived at: but the boy does not know what it is that has forced him to his conclusion; he does not know *the reasons* for his conclusion; so he usually invents a premise to account for it. This stage is illustrated by the following answers:

First Set of Answers. Henry W——. Set G I.

1. The sixpence was brighter than the half-crown because the sixpence might had been new.

First Set of Answers. Alfred D——. Set G I.

1. The sixpence is brightest brighter than the half-crown because the half-crown was made before the sixpence.

Then follows a stage which is represented more generally among children of the age dealt with in this research. The solution is quite clearly apprehended; but the reasons for it are not clearly and successively distinguished: the premises are combined in such a way as to be little more than restatements of the conclusion, or, more frequently still, one premise is given and the other omitted. These aspects are illustrated by the following:

First Set of Answers. Edward F——. Set G I.

1. The sixpence is brighter than the half-crown. Because the shilling is brighter than then hf-crown.

This boy states the two premises for the first time in his sixth and concluding paper. Another illustration follows:

First Set of Answers. Charles K——. Set G I.

1. The sixpence was brighter than the half-crown because if the sixpence was brighter than the shilling it would be brighter.

Charles is wavering on the border line of what has been accepted as an accurate answer to this problem for children of this age; for in his second paper he quotes both premises correctly, but in his third falls away again thus:

Third Set of Answers. Charles K——. Set G III.

1. Harry can write better Tom because Harry can write better than both.

In his fourth, fifth and sixth papers Charles quotes both premises quite correctly.

The next stage brings us to the correct solution with the premises clearly stated. This is the commonest form of answer for eleven-year-old boys, and is, as I have said, accepted as carrying a mark. I give one illustration:

First Set of Answers. William H——. Set G V.

1. Yes, it was brighter than the half-crown, because the sixpence was brighter than the shilling, and the shilling brighter than the H. crown. So the sixpence *must* be the brighter of the two

There is, of course, a higher type of answer than this; the reasoner may approximate to the general principle or 'form' of reasoning which is involved; thus

First Set of Answers. Alfred P——. Set G I.

1. The sixpence is brighter than the half-crown because if the shilling is brighter than it and the sixpence is brighter than the shilling it must be brighter than the half crown because the sixpence is brighter than the coin that is brighter than it.

The above, however, is the only answer I think in this series which approximates to some rendering of the general 'form' or principle on which the reasoning out of this problem is dependent.

ii. *The Development of the Answers to Problem* 2.

A preliminary stage is, as we found before in connexion with problem 1, the neglect or distortion of the premises.

First Set of Answers. Victor S——. Set G I.

1. they all had them all rite because why they were write because they tried

Then follows a stage in which the boys guess their solution of the particular case and support it by some reasonable hypothesis which they give as their reason for their answer, thus:

First Set of Answers. Alfred P——. Set G I.

2. I think the Standard III boy if he is an intelligent boy got all his sums right because if he is good at sums he would most certainly have got them right,

First Set of Answers. Henry W——. Set G I.

2. The boy in Standard III did not get his sums right because he do not think over his sums.

First Set of Answers. Alfred D——. Set G I.

2. I think a boy got all his sums right, because he shines out well.

First Set of Answers. Charles F——. Set G I.

2. I would think he would have them all right. Because he his in the Standard III

First Set of Answers. William H——. Set G I.

2. The boy in standard three named H. Ruppersberry I suppose got all of them because not long ago he won a first class prize.

First Set of Answers. William N——. Set G I.

2. No, he is a dunce, because he is a dunce.

First Set of Answers. Richard R——. Set G I.

2. The Standard III. boy would not get all his sums right, if he was a dull boy because he would not get them right and the sharp boy would

Fourth Set of Answers. Leonard S——. Set G IV.

2. I think he is because if some of the boys are good, I should think he is

Then follows a stage in which the child realises that he cannot tell, but gives a reason which is not the reason which, logically speaking, should compel him to this conclusion, thus:

Fifth Set of Answers. Leonard S——. Set G V.

2. I cant tell because I was not there to see.

First Set of Answers. George B——. Set G I.

2. We cant tell! The boy might have been backward in his sums and got none or a very few right. Or the boy might have been good a sums, and had them right, & he might have got them wrong.

Fourth Set of Answers. Richard J——. Set G IV.

2. *You cannot tell.* Perhaps if he was good at his other work I should say *yes*, but if he was bad I should say *no*.

After this comes the stage at which the child answers correctly that he cannot tell and fastens on the expression 'some' as the keynote of the answer. Such replies, as I have previously explained, carry a mark. Examples of this stage are:

First Set of Answers. Edward M——. Set G I.

2. I cannot tell. Because only *some* got *all* their sums right.

First Set of Answers. William H——. Set G I.

2. I can't tell, because he may have got all his sums right, or he may have not, because only *some* had all correct.

Finally, a stage is reached in which the boy realises, not only that an inference from 'some' to any particular is precarious, but that in order to justify a safe conclusion, the expression 'all' would have been required, thus:

First Set of Answers. William B——. Set G I.

2. I dont know because all the boys did not get all their sums right. He might have been one with all of them right or he might not.

Second Set of Answers. William P——. Set G II.

2. You cannot tell, because *SOME* is not *ALL*.

Fifth Set of Answers. William H——. Set G V.

2. I can't tell, because he may be a good one, or he may not, for *some* is not *all*.

iii. *The Development of the Answers to Problem* 3.

This question possesses little difficulty for adults, but young children and mentally undeveloped persons find several difficulties. They find it hard to hold the premises together mentally so as to place the three things in spatial relation to each other; there is a stage of mere confusion. Curiously enough, with one or two exceptions, the boys did not set out the positions one by one by drawing diagrammatically. Such a procedure would have been of much service to them in apprehending the relative positions, and might have helped them in their reasoned answers. The stage of mere confusion, however, is scarcely, if at all, represented among these eleven-year-old boys; but following are possible instances from the work of the weakest pupil in this eleven-year-old group:

First Set of Answers. Victor S——. Set G I.

3. suttan is on the north side of Burton because sutton is on the north side of Burton.

Fifth Set of Answers. Victor S——. Set G V.

3. on the Est side of the butcher is on the side of all.

Beyond this comes a stage in which one of two things is correctly placed and some reason is given, though it may only be an inference from one of the premises. An illustration follows:

First Set of Answers. Henry W——. Set G I.

3. Burton is on the north side because Sutton is on the south side of Bunton.

But there is a further difficulty in connexion with this problem of a serious nature for very undeveloped minds. Such persons regard position as place rather than position in the sense of direction and in this sense are coerced by an axiom of reasoning either expressed or unexpressed of the following kind,—'Two things cannot be on the same side of the same thing.' I found this in connexion with a somewhat similar test given to *all* the children of several schools, including, therefore, children some years younger than eleven. And this 'axiom' of reasoning is also found among the weakest, even of these eleven-year-old boys. Illustrations follow:

Second Set of Answers. Henry W——. Set G II.

3. The butchers shop is on the right hand side of bakers shop because the toy-shop is on left hand side bakers

Second Set of Answers. Charles F——. Set G II.

3. The butchers shop is on the left side of the bakers shop because the toy shop is on the right side of the bakers.

Third Set of Answers. John H——. Set G III.

3. The Bugle is west of the Wasp, because the Firefly is east of the Wasp, and the Bugle is east of the Firefly

First Set of Answers. William N——. Set G I.

3. Downham is in the south because two cannot be on the north side of Sutton.

Second Set of Answers. William N——. Set G II.

4. On the right hand, because two cannot be in the same place at once.

First Set of Answers. Henry S——. Set G I.

3. Downham is on the South side of Sutton because Burton is on the north.

Fifth Set of Answers. Henry S——. Set G V.

3. The grocers is on the east side of the butchers because two things be in the same place at the same time.

The transition towards a better form of answer is shown in the next example; though even here an indication may be seen of the conception that two things can scarcely be on the same side of the same thing, for George has to take Burton away before he feels safe about his conclusion, thus:

First Set of Answers. George B——. Set G I.

3. Yes, D. is on the north of S.. Because Burton is north of S. & take B. away (supposing) & it leaves D. on the north side of S.

But George has only stated one of his premises; consequently no mark is given, though it is obvious that he has clearly apprehended the three things in their relative spatial positions.

Then follow, as is usual, correct conclusions with the omission of one of the premises, or a combination of them which does not clearly show the reasonings involved, thus:

Fifth Set of Answers. Victor W——. Set G V.

3. On the west. Because both are on the west of the Butchers

Second Set of Answers. William B——. Set G II.

3. It is on the left hand of the bakers shop because the toy shop is on the left hand and so is the butchers shop.

Fourth Set of Answers. William B——. Set G IV.

3. She is on the left hand side because Mary is on the left hand side of Kate.

None of the answers to problem 3 quoted in this section is regarded as worth a mark on the system of marking adopted, which requires correct reasoning with a full statement of the premises as well as the correct conclusion. The answers that follow are satisfactory and receive marks:

First Set of Answers. William H——. Set G I.

3. On the north side of Sutton, because if Burton is north of Sutton, & Downham north of B. ∴. It can't be any side, *but* north.

First Set of Answers. William P——. Set G I.

3. Downham is on the north side of Sutton, because it is north of Burton, which is the north of Sutton.

First Set of Answers. Edward M——. Set G I.

3 {North of Sutton} Because if Burton is north of Sutton, and Downham north of Burton, Downham *must* be north of Sutton.

Third Set of Answers. William P——. Set G III.

3. The east side because the Bugle is on the east side of the Firefly, & the Firefly on the east side of the Wasp, the Bugle must be on the east side of the Wasp.

Finally there are one or two answers which begin to indicate some consciousness of the general 'form' of reasoning involved, thus:

Second Set of Answers. William H——. Set G II.

3. On the left hand side, because the Toy Shop is left of the Bakers and a Butchers is even still more left by being left of the toy shop. [*Then follows a small diagram indicating the positions of the Butcher's, Baker's and Toy Shop.*]

First Set of Answers. Alfred P——. Set G I.

3. Downham is on the north side of Sutton, because if Burton is on the north side and Downham is on the north of Burton Downham must be north of it because it is farther north than the town that is north of Sutton.

Sixth Set of Answers. Alfred P——. Set G VI.

3. It is underneath because it is underneath the one that is underneath the red one it is underneath both therefore it must be underneath the red one.

It is possible that the words 'above' and 'underneath' in question 3 of Set G VI are more statically positional, so to speak, than right- and left-hand, north and south etc., used in question 3 of Sets I, II, III, IV and V, which are rather directively positional. If this be so, on my analysis of the development of the answers to problem 6, the question as set does not quite correspond in difficulty with problem 6 of the previous sets; it could, I think, be made to do so by the substitution of 'above' and 'below' for 'on top' and 'underneath.' An analysis of the results gives some slight ground for the suggestion I have advanced, for five of the boys who are wrong in problem 3 of Set V are right in problem 3 of Set VI and one of these definitely states in answer to problem 3 of Set V that the two things cannot be on the same side of a third thing at the same time—a difficulty he does not feel in problem 3 of Set G VI, which he answers quite correctly. On the other hand two boys who are right in problem 3 of Set V are wrong in Set VI, so that I can scarcely say that there is, from a numerical aspect, more than a balance in favour of the analysis I have suggested. Of the remaining boys twenty-two have both the problems right and twelve have both of them wrong.

iv. *The Development of the Answers to Problem* 4.

First, we have the state of mind involved in an unsuccessful endeavour to apprehend the premises together, in which as usual the premises themselves become distorted and even implicitly denied, thus:

First Set of Answers. Victor S——. Set G I.

4. Jin had as much as Harry they all ad the same.

Fourth Set of Answers. Richard R——. Set G IV.

4. Jack had more sums right than James because James had three times as many sums right than William so Jack will have more

First Set of Answers. Thomas S——. Set G I.

4. Tom had more mistakes than Jim and Jim more than Harry and if tom had twice as many as Jim and Jim twice as many as Harry Tom had more than Jim.

First Set of Answers. William B——. Set G I.

4. Tom had less than Jim because if Jim had twice as many as Harry Tom must have had less than Jim.

First Set of Answers. John N——. Set G I.

4. Less because Harry had more than Tom and Tom less than Jim.

The last four answers quoted (and similar ones not cited) seem to indicate some sort of interference resulting from attempts to solve problem 1; but, in the absence of other tests in which this problem is given *before* problem 1, this statement is a suggestion merely. Erroneous association, as all first-rate teachers know, is a fertile source of error when passing from one 'form' of arithmetical problem to another, and it may operate here.

Another stage, or rather a more definitely erroneous step in the same stage, is illustrated by the following; the conclusion is still wrong and the premises distorted, thus:

First Set of Answers. Edward H——. Set G I.

4. Tom had more mistakes than Jim, because Tom had four times as many as Harry and Jim.

Second Set of Answers. Edward H——.. Set G II.

4. James had less than Jack, because Jame had a quarter of Tom and Jack had half.

Then follows a very rare case: the premises are right but the conclusion wrong, thus:

First Set of Answers. Leonard S——. Set G I.

4. If Tom had twice Harry's and Jim had twice, Tom had less than Jim because if Jim had twice Toms Tom had only half Jim's.

Leonard, of course, has inferred from his own erroneous conclusion a restatement of it in another form which justifies it.

Now follows, as is usual, the stage in which the correct conclusion is reached, but the reason is not given. An hypothesis is given; which, if it were true, would justify the conclusion, thus:

First Set of Answers. Henry W——. Set G I.

4. Tom had the same as Jim because they might had been two good splesers [*spellers*].

I find it difficult to say whether this is a higher or a lower stage than the one previously illustrated. Henry begins with the answer just cited, arrives at a wrong conclusion in his second paper and accounts for it by saying "Jack had wone some off of James," and in his third, fourth and fifth answers, replies in terms similar to those of John N——, cited above. In his last and concluding answer Henry reaches the right conclusion a second time but quotes only half the correct premises.

This answer exemplifies the next stage, namely, a right conclusion with an unsatisfactory reason, thus:

Third Set of Answers. Henry S——. Set G III.

4. Jack had just as many sums right as James because it says so in the question.

Henry has not stated the reasons, probably he does not know them; but there is a form of wrong answer which definitely states reasons and states them clearly, thus:

Fifth Set of Answers. Alfred B——. Set G V.

4. just as fast because James can run faster than Johnny and Harry can run faster than Johnny.

Fourth Set of Answers. George B——. Set G IV.

4. Smith St. was the same length as Warner St.! Because if Smith St. is longer than Sun St. and Warner St. is longer than Sun St. Smith St. and Warner St. are both the same length.

On evidence gained from similar questions given over a much wider field, I will ask the reader to accept that there is at or about this stage an 'axiom' of children's reasoning which runs thus—'If two things are both greater than a third thing these two things are equal.' Alfred illustrates this principle in the answer cited above.

In the next stage the conclusion is correct and the problem reasoned out correctly for a particular number. Had not this problem been somewhat easier than the preceding three, I should have been disposed to mark as correct such answers as the following:

First Set of Answers. Victor W——. Set G I.

4. Tom had the same number because if Harry had three Tom would have six and Jim would have six.

First Set of Answers. George B——. Set G I.

4. Tom had the same as Jim. Say H. had 1 mistake T. had 2. Jim. has 2.

It is of course a useful pedagogical device with weak reasoners to lead the way by means of particular instances such as the above; but it is not proof in the proper sense of the term, as those who are working with the newer forms of demonstrative geometry are beginning to find out. Nor do the boys themselves ultimately regard it as satisfactory, for they advance to a stage beyond, the stage at which marks are allowed, thus:

Second Set of Answers. George B——. Set G II.

4. James had the same number as Jack! *Because if James had half as many as Tom, and Jack had half as many as Toms they had the same amount.* Because they both had half of Tom.

But George B——, in his own person, exemplifies three stages of answer to problem 4 and his last cited answer can scarcely be considered conclusive as to the best form, seeing that he wavers in his fourth paper, though in his fifth and sixth he returns to this type of response. However, the great majority of the answers of these eleven-year-old boys are of this type and very few indeed, having decided upon it, ever leave it. A few illustrations follow:

First Set of Answers. Leonard N——. Set G I.

4. Tom has the same number of mistakes as Jim because Tom had twice Harry's mistakes and Jim had twice Harry's mistakes they both had twice Harrys ∴ they both had the same number.

First Set of Answers. Edward M——. Set G I.

4. {The same number} Because Tom and Jim BOTH had twice as many as Harry.

First Set of Answers. Samuel B——. Set G I.

4. Tom had the same as Jim because if Tom had twice as many as Harry And Jim twice as many they must have been equal.

Fourth Set of Answers. Samuel B——. Set G IV.

4. Smith St. is the same length as Warner St because they are both four times as long as Sun St.

Sixth Set of Answers. William P——. Set G VI.

4. Just as much because they both had been away six times as much as Harry.

First Set of Answers. Richard J——. Set G I.

4. No he did not, but he had the same because Tom was twice Harry and Jim was twice Harry so they are equal.

No higher stage was reached. I should expect clever adolescents to give the general principle or form of reasoning involved such as 'Halves or doubles of the same are equal.' They would not, I think, except in very exceptional cases, state the 'form' as 'Equal multiples of the same are equal'; this represents a later stage of development.

v. *The Development of the Answers to Problem* 5.

As the reader is already aware, this is by far the most difficult problem in the Reasoning Tests of Set G. A fully correct answer carries four marks, which are very rarely obtained. There is a stage in this problem at which the child does not comprehend the question, thus:

First Set of Answers. Victor S——. Set G I.

5. they could play cricket better than football is hard to play cricket is best some can play football and some can play cricket

But even Victor seems to grasp the question in his second paper, though he is far from answering it correctly. He writes:

Second Set of Answers. Victor S——. Set G II.

5. All the boys had all had all The sums rite and dictation they had There sums rite

To this answer he adheres throughout his remaining papers.

Then there is a stage of confused comprehension aptly represented by the following:

First Set of Answers. John H——. Set G I.

5. You cannot tell, because they would all be mixed.

John makes the same reply, in exactly the same words in each of his six answers to Problem 6.

Next we have a stage in which the solution is right, or partly right, but the reasons have not been comprehended in their relation to the solution. The boys invent a hypothesis which will account for their conclusion, thus:

First Set of Answers. Henry W——m. Set G I.

5. Some boys could play cricket and football because they might have seen other boys play football.

First Set of Answers. Henry W——r. Set G I.

5. There were some boys who could play cricket and. football, because they play in the playground, and we would not have many to play in the match for us.

Henry treats the school as his own and bases his reason on the difficulty of getting enough good boys to play in school-matches, unless some boys played both. Of course there might have been some who could play both, but the inadequate reason prevents us from allowing any mark for the possible case. In his later papers he advances or retreats to complete uncertainty and further illustrates the stage of a right or partly right conclusion with an inadequate ground.

Second Set of Answers. Henry W——r. Set G II.

5. I can't tell, because I don't know who the boys were who got there sums, and I dont know who the boys were who got their dictation right

To this answer Henry adheres during the rest of the series. A similar difficulty is felt by the following boy: he finds the information given by the premises inadequate for a conclusion, but does not cite the reasons.

Sixth Set of Answers. Leonard S——. Set G VI.

5. I can't tell because I am not there to see.

But in his fourth paper Leonard illustrates another stage in the development of this answer. He argues from the likelihood that proficiency in one means proficiency in another, thus:

Fourth Set of Answers. Leonard S——. Set G IV.

5. I think they could do both, because, I think if one could do drawing he could do needlework

This stage is also exemplified in the following:

First Set of Answers. Alfred B——. Set G I.

5. There were some who could play football and cricket because If you could play football you ought to be able to play cricket

I must confess that the stage above illustrated is not very abundantly represented among these eleven-year-old boys. It would be much commoner, I think, among inferior adolescent reasoners.

The first stage in overt analysis appears to be one in which the two groups of less than half are regarded as commencing one from one end of the class or school, the other from the opposite end. Even on this analysis the conclusion drawn concerning the other boys is not always right. For example:

First Set of Answers. Richard M——. Set G I.

5. There were some boys who could play cricket and football, because less than half could play cricket, and less than half football, and there were some boys to make up the halfs and I think these were the ones who could play cricket and football.

Third Set of Answers. Henry S——. Set G III.

5. I can't tell because the boys that made up the two full halves might be the who can do both.

First Set of Answers. Richard R——. Set G I.

5. Their were some boys who could play cricket and football because it says less than half at cricket and less than half at football so the must be some who could play both cricket and football and those are what are left.

So far no marks have been obtained; but we now enter the stages in which some analysis with a satisfactory reason is directly made, and a possible solution given; or one in which the boy says "I can't tell," and gives an adequate reason. These two cases seem distinct enough theoretically, but I am at a loss to say which comes first. Some boys begin with a partial analysis of cases and then proceed to "I can't tell": others start with uncertainty and end with a partial analysis. Moreover —a more important criterion—I cannot find a decided tendency in one direction rather than the other as I pass from the weaker to the abler boys. Illustrations follow:

First Set of Answers. Henry D——. Set G I.

5. I do not think any boys could play both, because there were less than half could play cricket and less than half the boys could play football.

First Set of Answers. William N——. Set G I.

5. Some not play both because there is quarter who could not play anything.

First Set of Answers. William H——. Set G I.

5. I suppose the first lot could play cricket and the next football and the other two lots less than the halves from the few that were less could play neither because

it says one lot could play cricket but not football and the other football but not cricket.

Sometimes the analysis into groups clearly starts from the same end of the class or school, thus:

First Set of Answers. Richard P——. Set G I.

5. Yes there was because Less than half could play cricket and less than half could play football so there must be. Because 2 nearly halfs is more than one half.

To a boy 'less than half' means just less than half and does not range from 1 through a quarter to $\frac{1}{2}n - 1$, as, in strict logic, I suppose it does. So Richard's answer is accredited with two marks, one for his possible case, and one for his reason. Another answer on the same lines follows:

First Set of Answers. Charles K——. Set G I.

5. There might be because those who played football might be the same boys who also played cricket.

The great majority of the boys, however, when commencing analysis, clearly start from the *opposite*, not the *same* ends of the school.

Very few boys who begin analysis, stop precisely where they began; they realise that the problem is more indeterminate than they thought and give another case, sometimes to the exclusion of their first one, or say "I dont know" and give an adequate reason. The cases of 'both' and of 'neither' have been illustrated; the case of 'either' is not very frequently given by itself. But it is illustrated by the following:

Second Set of Answers. Charles R——. Set G II.

5. I don't know because the boys who had sums right might have had Dictation wrong, and those who had Dictation right might have had sums wrong.

As I said above, I do not know whether the stage I am about to illustrate comes before or after some partial analysis; it certainly comes before a complete analysis, but often follows partial analysis, which the boy, later on, apparently feels has been too definite. Sometimes, however, it appears to precede even partial analysis. The following are illustrations of this type of answer:

Second Set of Answers. William McC——. Set G II.

5. Cant tell, because it said, less than hf got prize for reading, and less than half got prize for drawing.

First Set of Answers. Edward M——. Set G I.

5. [I cannot tell] because *less* than ½ were cricketers and *less* than half footballers.

The next stage is one of more complete analysis into possible cases with a reason or reasons given, and is illustrated by the following:

Second Set of Answers. Charles J——. Set G II.

5. I can't tell because they might have got all their sums and had some mistakes or ell got their sums wrong and got their dictation right, and perhaps got both wrong.

Charles has written out the possible cases of 'either' and 'neither,' but gives no reason for his solution; otherwise his answer is good. Somewhat more complete is the following:

First Set of Answers. Samuel B——. Set G I.

5. We cannot tell becaus if there were less than a half who could play foot-ball and less than a half who could play cricket there may have been some who could play both or there may have been some who could play neither so you cannot tell.

First Set of Answers. William H——. Set G I.

5. I can't tell, because there were less than half who could play 1. and less than half who could play the other. There may have been some who could do both or there may have not. So I can't tell.

'Not both,' of course, logically includes two cases, those of 'neither' and 'one and not the other'; but it is not desirable to mark a boy's answer as if he were aware of this; and in any case, he has not analysed 'not both.' Another good illustration, still, however, not quite complete, follows:

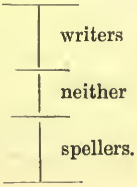
First Set of Answers. William P——. Set G I.

5. You cannot tell because less than half and less than half does not make 1 whole one, and one might play both, and one none at all.

Finally I quote one of the answers from the ablest reasoner in the school.

Third Set of Answers. Edward W. Set G III.

5. I can't tell because it says that Less than half were good spellers and less than half were good writers there might have been some boys who could do both or either or some who could do neither.



It is worth notice that his diagram refers to one case only, namely, that in which he started in thought from opposite ends, but he has included all the cases in his written answer.

This is not the place to enlarge upon the *pedagogical* advantages to be obtained from a consideration of analyses of development such as the foregoing; but perhaps a word may be said, in conclusion, on the *logical* and *philosophical* aspects of the investigation. It would appear that axiomatic certitude may attach itself quite well to principles of reasoning which, later on, the persons who use them will themselves see to be erroneous. It is probable that the passage to what we all call truth involves such errors in reasoning at one stage or another. I am not speaking of mere individual variation, but of common error. If this be so the word 'axiom' will acquire, and lose, meaning. It will acquire developmental significance, but its claim to 'eternal truth' will suffer a shock. There is no evidence that these boys applied their 'axioms' hypothetically. Though 'axioms' vary with development, they are never, as far as I have been able to discover amongst children, consciously held as postulates.

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THE CORRELATION BETWEEN GENERAL INTELLIGENCE AND PLAY ABILITY AS SHOWN IN ORGANIZED GROUP GAMES.

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1. *Introduction.*
2. *The groups of subjects on which the research was made.*
3. *Methods of investigation.*
4. *The methods used in finding the correlation coefficients, the results obtained.*
5. *Conclusions.*
6. *Summary.*

1. Introduction.

The value of Play as an important factor in education has been known and upheld by writers in all ages, and it is a significant fact that the Greeks, who rank as the nation that has attained the highest level of aesthetic development, based the larger part of their general scheme of education upon the Play Impulse.

During the middle ages the value of Play was discussed by many writers, but from the Greeks until the time of Froebel there is no record of any attempt to introduce a system of Education founded entirely on this impulse.

Since Froebel published his *Education of Man* the subject has received more and more consideration, and many of the great modern educational movements owe much of their success to the recognition of the importance of the Play Spirit.

It is not however until comparatively recent times that the ancient games of the people have become so thoroughly organized that they have reached a high stage of complexity and require very varied qualities in those who would excel in them. Games of this type such as football, cricket and hockey seem to appeal pre-eminently to the Anglo-Saxon, and the concensus of opinion goes to prove that participation in such games tends to train the qualities for which this race is renowned. The great popularity of football amongst those classes who have little opportunity for playing it—which leads to the phenomenally large attendance of spectators at matches—suggests that this game and others of the same type have been evolved to fit the special needs of the people, and that given opportunity, the proletariate would derive benefits from playing these games equivalent to those which have already been attained by the more favoured classes who have had such unlimited scope for participating in them.

Again the rapid growth of these organized games amongst women during the past twenty years provides an interesting opportunity for the study of their mental and physical effects, and in view of the beneficial results which appear to follow the introduction of handwork into schools it is not improbable that a complex game such as cricket or football involving as it does great general activity may improve brain growth. The proof of this would have far reaching effects on our system of education and might lead to the introduction of definite teaching of such games in all elementary schools.

In order to obtain such proof it seems important to investigate two problems:

(1) whether there is any correlation between ability to play such games and general ability,

(2) whether prolonged training in these games definitely affects mental development.

The object of this research has been to answer if possible the first of these two questions. The standard of play in hockey and netball in the case of girls, football, hockey and cricket in that of boys was taken as the basis for Play Ability.

Apart from the two national games for boys, hockey and netball were chosen as being most popular amongst girls.

The latter game was introduced from America some years ago and has grown so rapidly in popularity that it will probably supersede hockey. It is played with a ball similar to a football which is thrown from hand to hand of the team and finally into a goal formed of a net fixed to a ten foot pole. The game is very fast and involves speed and accuracy to a high degree.

The subjects for the investigation were chosen from very varied types of schools; at the same time every effort was made to keep the groups of individuals homogeneous. The tests were given in such a way that the ordinary school routine was interfered with as little as possible, and in all cases but one they were applied by the writer who had the advantage of being a teacher in one of the schools from which the groups were chosen, and had supervised school games for more than ten years.

In every case special care was taken to secure the most favourable experimental conditions to prevent extraneous factors such as excitement or strangeness of work interfering with the results obtained.

2. THE GROUPS OF SUBJECTS.

The number of subjects tested was approximately six hundred ranging from eight to eighteen years of age. The groups were taken from seven schools and were chosen with the object of testing children of different social status in order to eliminate the effects of tradition on the Play Ability.

Special work was done with children between the ages of twelve and sixteen as it has been held that it is during this period the greatest interest in group games arises. Up to the age of twelve a child is more individualistic in his play[1].

The greater part of the investigation was carried on with girls, partly because they were more accessible, but chiefly because girls are not so likely to be imbued with the group game habit as boys, and therefore offer clearer ground for research.

For the same reason groups were chosen which included children from elementary schools who had recently entered a secondary school where 'Games' formed part of the curriculum and were taught by a qualified teacher in school hours.

With few exceptions the children from the elementary schools began the group game on entering the secondary school and therefore gave scope for comparison with the children who had been in the school for some years and who had been playing the group game for some time previous to the investigation.

Groups I, II and III were drawn from London Schools, IV, V, VI, VII from Country Schools.

Group I. Sixty girls comprising Form L. IV of an endowed secondary school. The form was composed of two parallel classes and contained approximately 60 °/₀ of county scholars who had entered the school in the previous September and had therefore been in the school for two terms, and 40 °/₀ of children who had come up the school from the Kindergarten. The average age was 13 years.

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[1] Luther Gulick, *Ped. Sem.*, 1899, VI. Felix Clay, "Philosophy of Boys' Games," *Child Study*, IV.

Group II. Fifty-four girls of Form Up. IV in the same school. The constitution of the form was similar to that of Group I, but the majority of the children had been in the school at least one year and two terms. The average age was 14·5 years.

Group III. Fifty-eight girls of Form L. V in a County Secondary School. The girls had all been drawn from Elementary Schools and were in their second year in the Secondary School. The average age was 16 years.

Group IV. Sixty-nine girls from a private boarding school in the country. The ages ranged from 8—18 years of age. The children were drawn from the upper middle class.

Group V. Forty-seven boys from a private boarding school. The school was near the previous one and many of the brothers of the girls in Group IV attended it. The group included boys from 8—18 years of age.

Group VI. One hundred and fifty children attending a mixed Grammar School in a country town. The group consisted of seventy boys and sixty-nine girls the ages ranging from 8—18 years.

Results were obtained from this school in July 1912, and six months later (VI_1, VI_2). The experimental tests were only applied in the case of VI_2.

Group VII. Twenty-four girls from Classes V and VI of a boarding school for the daughters of professional men. Age 16—18 years.

The results were obtained from all the groups except VI Jan.—April 1912.

General Conditions.

Every effort was made to secure homogeneity in each of the groups. The conditions for Play were as uniform as possible. In Groups I and II the investigator was responsible in a large measure for their supervision. In Groups III, IV, VI, and VII the teachers who organized the games were personally known to the writer, were keenly interested in the subject and took great pains to make their reports reliable. In Group V the Headmaster himself collected the data required.

In Groups I, II, and III the subjects were of uniform age, under similar school conditions, and in the majority of cases drawn from the same social class so that their environment was very homogeneous.

In Groups IV, V, VII the heterogeneity resulting from differing age and class was counterbalanced by the fact that these were drawn from boarding schools where the conditions were very uniform. The home life of the children in these groups was also likely to be very similar and the 'games tradition' well established.

Group VI was made up of the children drawn from the middle class of a small country town.

3. METHODS OF INVESTIGATION.

The data obtained were derived from three sources:

(i) The Opinions of the Teachers.
(ii) School Marks and Form Positions.
(iii) Psychological Tests.

(i) *The Opinions of the Teachers.*

Much discussion has taken place as to the value of the teachers' opinion in measuring general intelligence. The arguments in favour seem considerably to outweigh those against, and the correlation coefficients between the teachers' estimate and examination marks are sufficiently high to lead to the conclusion that this method of estimating intelligence is very reliable.

The close approximation of the results obtained in this investigation to those of Karl Pearson[1], H. Waite and W. H. Gilby is shown in the following table:

Correlation Coefficients for Teachers' Estimate of General Intelligence v. *School Marks.*

Miss G. Jones and Karl Pearson	$r = \cdot 70$
H. Waite	$r = \cdot 689$
W. Gilby	$r = \cdot 686$
M. J. Reaney	$r = \cdot 62$ $r = \cdot 55$ $r = \cdot 52$

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[1] "On the Value of the Teachers' Opinion of the General Intelligence of School Children" (compiled from tables and reductions by G. Jones), Karl Pearson, *Biometrika*, vii. 542. "The Teachers' Estimate of the General Intelligence of School Children," H. Waite, *Biometrika*, viii. 79. "On the Significance of the Teachers' Appreciation of General Intelligence," W. H. Gilby, *Biometrika*, vii. 94.

The Reliability Coefficients obtained from two independent opinions are also, with one exception, high:

Groups	I,	II,	III,	IV,	V,	VI.
Coefficients	·79,	·43,	·74,	·81,	·70,	·6.

The low coefficient in Group II may probably be explained by the fact that the subjects on which the estimates were based were English and Science. In each case the two opinions chosen for comparison were those of the teachers who had most experience with the children.

Method. In each of the groups several independent estimates of intelligence were obtained. The average of these was taken and the correlations worked out from this average. The views on Play Ability were obtained from the teachers who supervised the games and from the Games Captains. (See Table I.)

TABLE I.

Groups	Number of opinions	Subjects on which estimates were based	Remarks
I	School subjects 5 Games 5	English, Languages, History, Maths., Science	The games were supervised by three mistresses and by the games captains
II	School subjects 4 Games 5	English, History, Maths., Science	As above
III	School subjects 4 Games 1	English, Languages, Maths., Geography	The games were taught by a specially qualified games mistress
IV	School subjects 5 Games 1	English, Languages, History, Geography, Science	The games were taught by a special games mistress
V	School subjects 3 Games 4	All school subjects	The games were supervised by four masters
VI	School subjects 3 Games 3	English, Languages, Geography, Maths., Science	One master was responsible for the games
VII	School subjects 7 Games 1	English, Geography, History, Languages, Maths., Science	The games were taught by a special games mistress

Each teacher was asked to give his or her views of the child's general ability. These views were based on knowledge of character and of the standard of work in the subject taught. Capacity rather than achievement was judged.

The following system of marking was carefully explained, the terms used although chosen independently are very similar to those adopted by Prof. Karl Pearson[1].

Very good (V.G.) = exceptional intelligence or ability.
Good (G.) = well above average intelligence.
Medium + (M. +) = just above average intelligence.
Medium (M.) = average.
Medium – (M. –) = just below average intelligence.
Poor (P.) = well below average.
Very Poor (V.P.) = exceptionally low ability.

The views on ability in play were given after careful consideration of the following points:

(*a*) Length of time the child had learnt the game,
(*b*) Motor coordination, alertness, precision, etc.,
(*c*) Mental grasp of the game.

(ii) *School Marks and Form Positions.*

It was only possible to get these data in Groups I and II. The marks for two consecutive years were obtained in each case and were percentaged to allow for absence.

(iii) *Psychological Tests and their Application to the Subject.*

The tests used in the investigation were chosen chiefly because of the ease with which they could be applied to a large number of subjects simultaneously. They were treated as part of the ordinary school course and great care was taken to alter the general routine as little as possible.

The children were led to believe that the special work was a test in connexion with their ability to play games. They showed great interest and appeared to enjoy it thoroughly.

The tests were applied three times in Groups I, II, and III, a fortnight's interval being allowed between each application. In Groups IV, V, and VI the tests were applied twice with an interval of three days.

The tests were always taken in the order in which they are numbered, at the same time of day and in the case of the first three groups on the same day of the week.

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[1] Karl Pearson. "On the Relationship of Intelligence to Size and Shape of Head and to other Physical and Mental Characters," *Biometrika*, v. 105.

With one exception they were applied by the investigator, the Form Master or Mistress remaining in the room. In Group V the second series of tests was applied by the Headmaster who had been present when the first series was taken.

In each case the results of the sets of tests were combined and an average obtained from which the correlation coefficients were worked out.

The following is a list of the tests used:

		Name used in Scheme.
1.	Crossing out every letter in a page of print	Motor Test.
2.	Crossing through the letters 'e' and 'r.'	e r Test.
2_a.	Crossing through the letters 'a n o s.'	a n o s Test.
3.	Ringing sets of dots.	A Test.
4.	Choosing and ringing sets of dots.	B Test.
5.	Schema or Spot Pattern Test.	Spot Pattern Test.

Before each test a careful blackboard demonstration and very detailed directions were given to every group, and in no case was the test applied until each child understood clearly what to do.

Tests 1 *and* 2. This is a well-known test and consists in crossing through various letters in a page of print. It was originated by Bourdon[1], who used it to measure discrimination. Modifications of it were used by Oehrn[2] to measure attention, by Cattel and Farrand for rate of perception, by Thorndike[3] for estimating the relation between the mental traits of twins, and by Winteler and Ritter for testing fatigue. Binet[4] used it as a test for intelligence, Vogt[5] to measure attention and distraction, and Whipple[6] to measure fatigue and class standing. In more recent investigations modified forms of the cancellation test have been used by William Brown[7], Burt[8] and Wyatt[9] in testing general intelligence.

[1] B. Bourdon. "Observations comparatives sur la reconnaissance, la discrimination, et l'association," *Rev. philos.* 1895, xl. 153—185.

[2] A. Oehrn. "Experimentelle Studien zur Individualpsychologie," *Psychol. Arb.*, 1896, i. 92—151.

[3] Thorndike. "Measurement of Twins," *Arch. of Philos.*, 1905, i.

[4] A. Binet. "L'étude expérimentale de l'intelligence," Paris, 1903, 309.

[5] R. Vogt. "Ueber Ablenkbarkeit und Gewöhnungsfähigkeit," *Psych. Arb.*, 1901, iii. 62—202.

[6] G. Whipple. *Manual of Mental and Physical Tests*, 255.

[7] W. Brown. This *Journal*, 1910, iii. 296—322. Cf. also *Mental Measurement*, Cambridge, 1911.

[8] C. Burt. "Experimental Tests of Higher Mental Processes and their Relation to General Intelligence," *J. of Exp. Ped.*, 1911, i. 93—112.

[9] S. Wyatt. "The Quantitative Investigation of Higher Mental Processes," this *Journal*, 1913, vi. 113.

In the following table the results obtained in this investigation are compared with those of the last three writers mentioned:

Table II. *Correlation between the Tests and the General Intelligence of the Subjects.*

Test	Brown	Burt	Wyatt	Reaney
e r	0·00	·39	·40	0·0
	·28		·37	·3
				0·0
				·2
				·13
				·22
Motor	·32			·5
	·13			·36
				·1
				·1
				·12
				·08

Note. Whipple found an inverse correlation with intelligence for erasure of four letters.

The method adopted in applying these tests was identical with that used by Brown in a previous investigation[1].

Test 1. *The Motor Test.* Pages of French words arranged in irregular order were employed. The subject was told to cross out every letter separately. The page was given out face downwards. At a given signal the subject turned it over and on the word "begin" started crossing out every letter with single strokes, continuing until the signal "stop" was given. The time allowed for this test was three minutes. A different set of words was employed for the second test. The subjects were advised to go as quickly as possible but not to pass over any letters and to work from left to right.

System of Marking:

+ 1 mark for every letter crossed out correctly.
− 1 mark for every letter passed over.

Test 2. *The e r Test.* The method of procedure was identical with that of test 1, but the subjects were told to cross out the letters 'e' and 'r' only. Pages of French print were taken which gave approximately the same number of e's and r's in each line.

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[1] This *Journal*, 1910, iii. 296—322.

System of Marking.

+ 1 mark for every letter crossed out correctly.

− 1 mark for every letter crossed out wrongly or passed over.

Test 2_a. *The a n o s Test.* The test was identical with the above but the letters used were a n o s. This test was applied instead of the e r test to the older members of Groups IV and V. The results are included in those of the e r test.

Test 3. *A Test*[1]. In this test a series of dots were arranged in clearly defined sets of three (see page 236, Fig. 1 A). The subject was told to ring the sets as quickly as possible. The paper was given out face downwards, it was turned over at a signal and at the word "begin" the children started ringing the dots as fast as they could. The one who finished first was told to hold up a hand. At this a signal to stop was given and the pencils were immediately put down.

Test 4. *B Test*[1]. This test was similar to the A test, but the dots were arranged indiscriminately (see page 236, Fig. 1 B), and the subject had to pick out the sets of three.

System of Marking for the A and B test.

+ 1 mark for every ring.

− 1 mark for every dot omitted down to the point where the rings stopped.

Test 5. *Schema or Spot Pattern Test.* In this test the left hand column of patterns (see page 237, Fig. 2) was taken as the model. The papers were given out face downwards. The subjects were allowed to look for a few seconds to note the position of the copy and at a given signal turned the paper over and began to cross out the extra dots in order to make the patterns in each line like the copy. The one who finished first put her hand up and the rest of the class were immediately told to stop. The same set of papers was given for the second test except in Group V, where a more difficult one was substituted.

System of Marking.

'x' marks (scale shown in diagram) for every correct pattern.

'0' marks for every incorrect pattern.

In tests III, IV and V the time taken on the second occasion was naturally shorter and the marks were adjusted to the same standard.

A marked improvement, due to practice, was noticed in all cases in the second test.

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[1] These tests were devised by E. Bradford and were based on MacDougall's tachistoscopic test.

Factors involved in Play Ability for a Group Game, such as Hockey.

The following is a short summary of what seem to be the most marked qualities required in playing a game such as hockey:

(*a*) Motor coordination of muscles of eye, limbs and body.

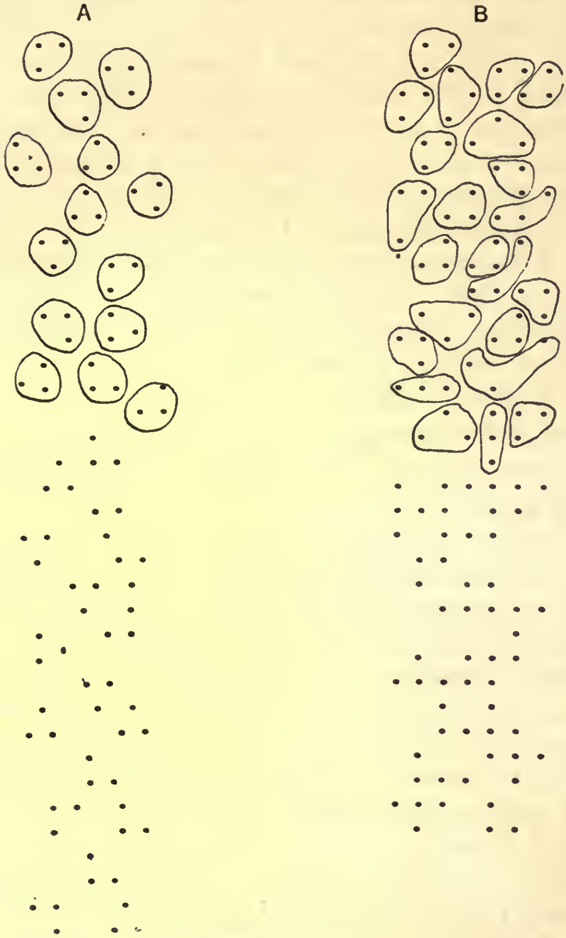

Fig. 1.

(*b*) Quickness of response to stimuli.

(*c*) Accuracy in judging distance, and force required to send the ball.

(*d*) Sense of direction, motor and sensory discrimination.

(*e*) Memory of past experience and power of holding the distribution of the team in mind.

(*f*) Endurance, concentration and attention.

(*g*) Ideation, power of keeping an end in view.

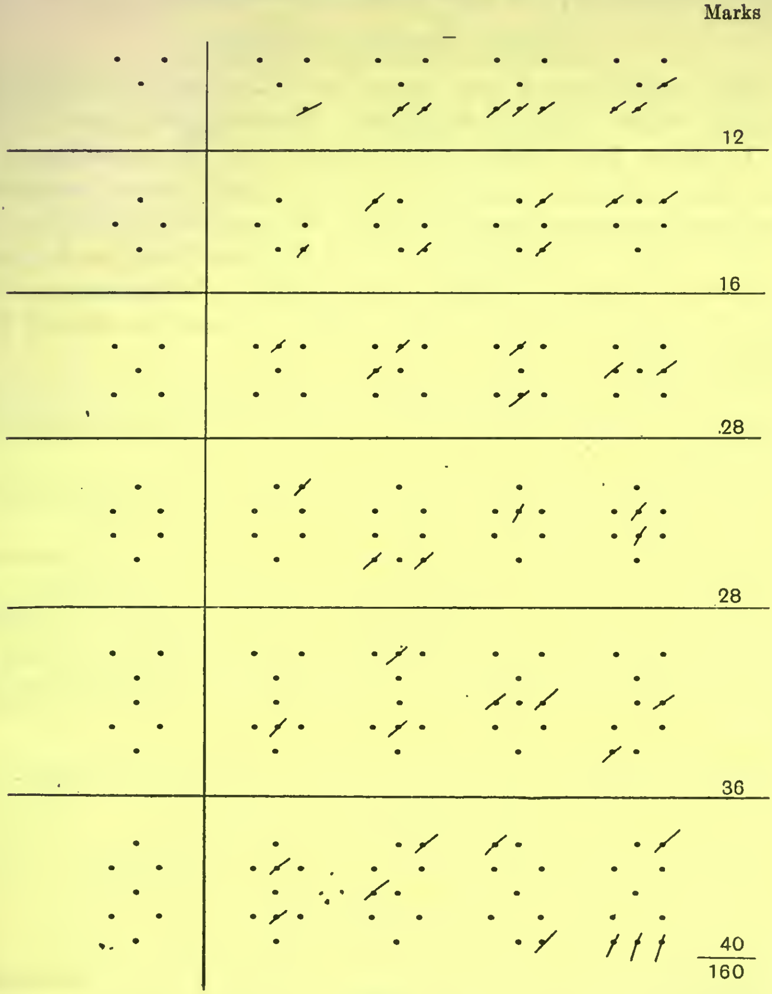

Fig. 2.

(*h*) Group consciousness, involving power of acting as part of a whole and therefore bringing into play self control.

(*i*) Reasoning power and judgement.

(*j*) Imagination in picturing the redistribution of the field as the result of certain play.

(*k*) Strong volitional power, involving self control, power of making a prolonged effort and of keeping cool in an emergency.

Of the qualities described above, all save the first four seem to involve intelligence to a greater or less degree.

General Remarks on the Tests.

The tests described were chosen as making use of some of the simpler qualities required in playing a complex group game.

1. *Motor Test.* This certainly brings into play coordination of hand and eye muscles, control of muscles of hand and arm, endurance, concentration, attention and promptness of response to a stimulus.

2. *e r Test.* This test makes use of the same factors as the motor test, but in addition requires memory, power of discrimination, and power to inhibit muscular action. Volition is also brought in to a certain extent.

3. *A Test.* This makes use of the same factors as the motor test, but greater freedom of movement is allowed so that larger muscles come into play.

4. *B Test.* This test is similar to the A test, but requires greater power of discrimination.

5. *Schema or Spot Pattern Test.* This test requires less muscular coordination but makes special use of memory, concentration and attention, also power of apprehension of form and grouping. Bradford claims that it tests the span of apprehension, the subject having to carry the copy of the pattern in his head in order to get the work done to time[1].

4. THE METHODS USED IN FINDING THE CORRELATION COEFFICIENTS AND THE RESULTS OBTAINED.

In the present investigation the method of ranks formula

$$\rho = 1 - \frac{6\Sigma (d^2)}{n(n^2-1)},$$

where Σ = symbol for summation, d is the difference between the rank of an individual in one series and his rank in the next, and r is the number of individuals examined, with Pearson's[2] correction

$$r = 2 \sin \left(\frac{\pi\rho}{6}\right),$$

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[1] *School World*, April 1912.

[2] Karl Pearson. *Drapers' Company Research Memoirs. Biometric Series*, 1907, iv. 18.

was used in the majority of cases as being the most applicable to the material in hand. In order to test the reliability of this method several cases were worked out independently by the product moment formula with the following results:

TABLE III. *Correlation Coefficients.*

Method of ranks	Product moment formula
$r = \cdot 79 \pm \cdot 03$	$r = \cdot 71 \pm \cdot 03$
$\cdot 63 \pm \cdot 03$	$\cdot 66 \pm \cdot 03$
$\cdot 86 \pm \cdot 02$	$\cdot 90 \pm \cdot 02$
$\cdot 39 \pm \cdot 09$	$\cdot 37 \pm \cdot 09$
$\cdot 59 \pm \cdot 06$	$\cdot 60 \pm \cdot 06$

In the case of Group VI where possible the correlation was obtained by means of a correlation table, when one set of data was qualitative, η was found instead of r and when both were qualitative the contingency table was used.

The Probable Error. The probable error was found by the formula $\text{P.E.} = \frac{\cdot 7063\,(1 - r^2)}{\sqrt{n-1}}$, where r = correlation coefficient and n = number of cases referred to.

When the probable error was greater than the correlation coefficient the correlation was taken to be 0. The cases in which the coefficients were greater than three times the P.E. and greater than twice the P.E. were also noted.

Special Marking. To ensure that the rank method should be as accurate as possible, Karl Pearson's[1] method of marking was used in which the rank given to a set having the same marks is the middle rank of the set.

The full list of correlation coefficients will be found in Table V.

The Age Factor. Games Ability and Age. In Groups I, II and III, the age was constant; in the other groups it varied, and in these there seems to be a definite correlation between Age and Play Ability. (In Group IV $r = -\cdot 115$, in Group V $r = \cdot 28$, in Group VI $r = \cdot 25$, in Group VII $r = \cdot 2$.) This might be accounted for by the length of time the older children had spent in the school and the greater experience in games resulting from it. The negative correlation in Group IV may result from a possible tendency of the teachers to estimate the ability of the younger children more leniently.

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[1] Karl Pearson. *Biometrika*, April, 1910.

Intelligence and Age. Very slight correlation is shown (r in Group IV $= -0{\cdot}6$, in Group VI $= 0$, in Group VII $= {\cdot}18$), with the exception of Group V (where $r = -{\cdot}22$). This may be due to the same lenient tendency amongst the teachers towards the younger children.

Correlation of Age with Tests. The motor test shows no correlation except in Group V ($r = {\cdot}42$). The e r test shows a distinct correlation (r in Group IV $= {\cdot}6$, in Group V $= {\cdot}41$, in Group VI $= {\cdot}24$). The A test yields a correlation coefficient of ·41 in Group IV but no correlation is revealed in the case of the other groups. The B test gives varied results (r in Group IV $= 0$, in Group V $= {\cdot}3$, in Group VI $= {\cdot}7$). For the Spot Pattern $r = {\cdot}3$ in Groups IV and VI, $= 0$ in Group V.

Elimination of the age factor. The age factor was eliminated for Games Ability and General Ability by partial correlation, the formula used[1] being $r_{12\cdot3} = \dfrac{r_{12} - r_{13}\,r_{23}}{\sqrt{1 - r_{13}^2}\,\sqrt{1 - r_{23}^2}}$.

TABLE IV. *Correlation between Games and Intelligence. Age Constant.*

	Partial Cor.	Entire Cor.
Group IV	·28	·28
Group V	·36	·25
Group VI	·35	·35
Group VII	·45	·39

Age was also eliminated for Games Ability and Tests (see Table VI), and for the tests themselves in Group VI. In the latter case there are only two results which show a change in the correlation coefficients. To test these the parallel cases were taken in Groups IV and V. These showed no change.

From the results obtained it is admissible to assume that age has practically no effect on the data investigated.

Elimination of other 'Irrelevant' Factors. Many of the results of the earlier work on correlation of mental characteristics have been shown to be almost useless because of the heterogeneity of the material employed, and much of the more recent research on the subject has brought into prominence the great necessity for the homogeneous condition of such material.

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[1] G. Udny Yule. "On the Theory of Correlation for any Number of Variables treated by a new System of Notation." *Proc. Roy. Soc.*, 1907, LXXIX.

Table V. *Partial Correlations.*

Groups	I	II	III	IV	V	VI	VII
G *v.* I Ag	—	—	—	·27 ± ·075 *·28*	·36 ± ·08 *·25*	·35 ± ·05 *·35*	·45 *·39*
G *v.* M Ag	—	—	—	·31 ± ·07 *·3*	·03 ± 1 *·15*	·13 ± ·055 *·19*	—
G *v.* e r Ag	—	—	—	·55 ± ·05 *·36*	·3 ± ·09 *·29*	·23 ± ·052 *·25*	—
G *v.* A Ag	—	—	—	·29 ± ·07 *·2*	·08 ± 1 *·12*	·29 ± ·05 *·3*	—
G *v.* B Ag	—	—	—	·2 ± ·08 *·2*	·13 ± ·09 *·21*	·14 ± ·055 ·2	—
G *v.* Sp Ag	—	—	—	·36 ± ·07 *·29*	·01 ± 1 *·14*	·14 ± ·055 *·18*	—
G *v.* e r M	·001 *·08*	·34 ± ·08 *– ·01*	·01 *·1*	·02 *·36*	·02 *·29*	·17 *·25*	—
G *v.* A M	·3 *·29*	·31 *·31*	·3 *·3*	·16 *·3*	·08 *·12*	·03 *·3*	—
G *v.* B M	·03 *·15*	·104 *·04*	·07 *·12*	·04 *·2*	·13 *·21*	·12 *·2*	—
G *v.* Sp M	·3 *·23*	– ·2 *– ·2*	·14 *·14*	·18 *·29*	·12 *·14*	·05 *·18*	—
G *v.* B A	– ·03 *·15*	– ·09 *·04*	– ·01 *·12*	·0 *·2*	·16 *·21*	– ·03 *·2*	—
G *v.* Sp A	·15 *·23*	– ·13 *–·129*	·05 *·14*	·09 *·29*	·106 *·14*	0 *·18*	—
G *v.* A I	·3 *·29*	·32 *·31*	·3 *·3*	·25 *·3*	·06 *·12*	·3 *·3*	—
G *v.* B I	·05 *·15*	—	—	·1 *·2*	·17 *·21*	·13 *·2*	—
G *v.* Sp I	·21 *·23*	—	·12 *·14*	·23 *·29*	·12 *·14*	·15 *·18*	—
G *v.* e r I	·11 *·08*	·04 *– ·01*	·01 *·1*	·32 *·36*	·16 *·32*	·17 *·25*	—
Int *v.* A G	– ·2 *·0*	·0 *– ·07*	·0 *·02*	·14 *·23*	·22 *·25*	— *0*	—

Total correlations in italics for comparison.

Use of terms.

G = Games Ability
Ag = Age
I = Intelligence
M = Motor Test
e r = e r Test
A = A Test
B = B Test
Sp = Spot Pattern Test

In order to eliminate certain irrelevant factors in the present investigation the following analysis was made of those groups in which such factors might occur and the correlation coefficients were found independently for the subgroups.

Group I. This group was made up of two parallel classes and contained approximately 60°/$_0$ of children who had just left elementary schools, and 40°/$_0$ of children who had been in the school for longer than two years.

TABLE VI. *Elimination of Age. Group VI.*
Correlation Coefficients for all tests. Age Constant.

	Games A	Int.	Motor	A	B	e r	Spot P.
Games	—	·35 (·35)	·13 (·19)	·29 (·3)	·14 (·2)	·23 (·25)	·14 (·18)
Int.	·35 (·35)	—	·15 (·12)	0	·2 (·2)	·28 (·22)	·3 (·3)
Motor	·13 (·19)	·15 (·12)	—	·83 (·9)	·09 (·5)	·66 (·66)	·76 (·77)
A Test	·29 (·3)	0	·83 (·9)	—	·48 (·74)	·7 (·7)	·54 (·6)
B Test	·14 (·2)	·2 (·2)	·09 (·5)	·48 (·74)	—	·7 (·7)	·8 (·8)
e r	·23 (·25)	·28 (·22)	·66 (·66)	·7 (·7)	·7 (·7)	—	·42 (·46)
Spot P.	·14 (·18)	·3 (·3)	·76 (·77)	·54 (·6)	·8 (·8)	·42 (·46)	—

Total Correlation Coefficients in brackets for comparison.

Tests to compare results in Groups IV, V, VI.

	Motor *v.* B. Age const.		A *v.* B. Age const.	
IV	·55	(·55)	·72	(·72)
V	·54	(·69)	·6	(·6)
VI	·09	(·5)	·48	(·74)

The following subgroups were made:

Group I$_a$ = Class$_a$.
Group I$_b$ = Class$_b$.
Group I$_c$ = Children from elementary schools.
Group I$_d$ = Children who had been in the school for more than two years.

TABLE VII. *Table of Correlation Coefficients.*

Group	I $N=60$	II $N=54$	III $N=56$	IV $N=69$	V $N=47$	VI $N=148$	VII $N=24$
ames Ab. *v.* Games	·57 ± ·06	·5 ± ·078	Not obtainable	Not obtainable	·7 ± ·05	Not obtainable	
ames *v.* Intelligence	·56 ± ·06	·18 ± ·09	·09 ± ·09	·28 ± ·07	·25 ± ·09	·34 ± ·05 (1) ·35 ± ·05 (2)	·39 ± ·12
ames *v.* Marks	·45 ± ·07	·21 ± ·09	—	—	—	—	
ames *v.* Motor Test	·08 ± ·09	·03 ± ·1	·12 ± ·08	·3 ± ·06	·15 ± ·098	·19 ± ·05	
ames *v.* e r Test	·08 ± ·09	– ·01 ± ·1	·1 ± ·09	·36 ± ·06	·29 ± ·09	·25 ± ·05	
ames *v.* A Test	·29 ± ·08	·31 ± ·085	·3 ± ·08	·3 ± ·06	·12 ± ·098	·3 ± ·05	
ames *v.* B Test	·18 ± ·09	·04 ± ·1	·12 ± ·09	·2 ± ·07	·21 ± ·09	·2 ± ·05	
ames *v.* Spot P. Test	·23 ± ·08	– ·2 ± ·09	·14 ± ·09	·29 ± ·07	·14 ± ·08	·18 ± ·05	
ames *v.* Age	Age constant	Age constant	Constant	– ·115 ± ·08	·28 ± ·09	η = ·25	·2 ± ·13
ıtelligence *v.* Int.	·79 ± ·03 *·71 ± ·04*	·43 ± ·08	·74 ± ·03	·81 ± ·02	·7 ± ·05	(·44 ± ·04) Sc. & English c = ·60 Sc. & Maths.	
ıt. *v.* Marks	·52 ± ·06	·62 ± ·06	—	—	—	—	
ıt. *v.* Motor Test	– ·5 ± ·06	– ·36 ± ·08	– ·1 ± ·09	·08 ± ·08	– ·1 ± ·1	·12 ± ·055	
ıt. *v.* e r Test	0	– ·3 ± ·08	– ·07 ± ·09	·2 ± ·07	·13 ± ·09	·22 ± ·05	
ıt. *v.* A Test	0	– ·07 ± ·1	·02 ± ·09	·23 ± ·07	·25 ± ·09	0	
ıt. *v.* B Test	·2 ± ·08	– ·1 ± ·095	·05 ± ·09	·37 ± ·07	·2 ± ·09	·2 ± ·054	
ıt. *v.* Spot P. Test	·09 ± ·09	·05 ± ·1	·22 ± ·08	·25 ± ·07	·16 ± ·09	η = ·1	
ıt. *v.* Age	Age constant	Constant	Constant	– ·06 ±	– ·22 ± ·09	0 η = ·09	·18 ± ·13
[otor *v.* Motor	—	·47 ± ·08	—	·67 ± ·045	·8 ± ·055	*·8 ± ·02*	
[otor *v.* e r	·08 ± ·09	·33 ± ·09	·04 ± ·09	·6 ± ·05	·55 ± ·07	·63 ± ·03 *·66 ± ·03*	
[otor *v.* A	·64 ± ·05	– ·08 ± ·1	·42 ± ·07	·65 ± ·05	·66 ± ·06	*·9 ± ·01*	
[otor *v.* B	·27 ± ·08	– ·2 ± ·09	·42 ± ·07	·55 ± ·05	·69 ± ·06	*·5 ± ·04*	
[otor *v.* Spot P.	– ·1 ± ·09	·02 ± 1	·02 ± ·09	·44 ± ·064	·18 ± ·09	*·76 ± ·024*	
[otor *v.* Age	Constant	Constant	Constant	·06 ± ·08	·42 ± ·085	*·03 ± ·06*	
r *v.* e r	—	—	—	·83 ± ·03	·86 ± ·025 *·9 ± ·025*	*·9 ± ·01*	
r *v.* A	·13 ± ·09	– ·1 ± ·095	·24 ± ·08	·5 ± ·05	·63 ± ·06	*·7 ± ·024*	
r *v.* B	·11 ± ·09	– ·1 ± ·095	—	·6 ± ·03	·74 ± ·04	*·7 ± ·024*	
r *v.* Spot P.	—	– ·4 ± ·085	– ·3 ± ·08	·45 ± ·06	·33 ± ·09	*·46 ± ·04*	
r *v.* Age	Age constant	Constant	Constant	·6 ± ·045	·41 ± ·085	*·24 ± ·05*	
v. B	·6 ± ·07	·77 ± ·05	·75 ± ·036	·62 ± ·045	·74 ± ·04	*·7 ± ·024*	
v. Spot P.	·34 ± ·08	·03 ± ·1	·29 ± ·08	·34 ± ·07	·34 ± ·085	*·6 ± ·03*	
v. B	·61 ± ·07	·39 ± ·09 *·37 ± ·09*	·66 ± ·05	·72 ± ·038	·6 ± ·06	·74 ± ·024	
v. Age	Age constant	Constant	Constant	·41 ± ·07	·15 ± ·098	– ·14 ± ·05	
v. B	·43 ± ·07	·93 ± ·01	·57 ± ·065	·52 ± ·06	·7 ± ·05	*·9 ± ·01*	
v. Spot P.	·32 ± ·08	·1 ± ·095	·26 ± ·04	·32 ± ·07	– ·21 ± ·06	*·8 ± ·02*	
v. Age	Age constant	Constant	Constant	·02 ± ·08	·3 ± ·09	*·7 ± ·024*	
pot *v.* Spot	·55 ± ·05	·67 ± ·05	·61 ± ·058	·48 ± ·06	·59 ± ·06	*·9 ± ·01*	

roup	I					II					III		VI$_1$		VI$_2$	
ıb-group	*a*	*b*	*c*	*d*	*f*	*a*	*b*	*c*	*d*	*f*	*a*	*b*	*x*	*y*	*x*	*y*
	$N=32$	$N=28$	35	25	57	28	26	36	18	42			70	69	80	74
ımes *v.* Int.	·7	·4	·6	·6	·32 ± ·08	·18	·21	·32	·01	·22	– ·03	·18	·34	·42	·33	·28
ımes *v.* Marks			·52	·46				·14	·26	·21						

$_1$ and VI$_2$=consecutive years whole school.

$_x$=boys, VI$_y$=girls.

=class I, I$_b$=class II.

=elementary scholars, I$_d$=scholars who have come up through the school.

I$_f$=2nd year results with same children.

II$_{a,\ b,\ c,\ d}$=as in I.

Cor. coefficients in italics by product moment formula.

All other coefficients by method of ranks corrected.

Group I_f = The same group a year later.

Group II. This consisted of two parallel classes and was made up of children drawn from elementary schools who had been in the school one and a half years, and children who had been more than three years in the school. A similar analysis was made to that in Group I.

Group III. This was composed of two parallel classes and the subgroups were therefore III_a and III_b.

Group VI_1. This was a mixed group and was therefore subdivided for sex into VI_x = boys, VI_y = girls.

Group VI_2. The same school taken six months later.

The results of this analysis into subgroups are given in Table VII.

Analysis of Results.

In the analyses of Groups I and II which were taken from the school in which the writer had taught for some years some interesting results are to be noticed. The subgroups *c* and *d* of Group I give identically high correlations (r = ·6); in subgroup *a*, r = ·7, while subgroup *b*, r = ·37, approximates more closely to the median. Independent knowledge of this group showed that class *a* was unusually keen in games, and was the victor in the school netball tournament, while class *b* were good average players. The whole group taken one year later gives a correlation identical with the median (r = ·32).

TABLE VIII. *Correlation Coefficients for Play Ability* v. *Intelligence. Average Coefficient* ·32.

Groups	Coefficients	Groups	Coefficients
VI_1	·34	II_b	·21
VI_2	·35	VI_{1y}	·42
VI_{1x}	·34	I	·56
VI_{2x}	·33	I_d	·6
II_c	·32	I_c	·6
VII	·39	I_a	·7
I_b	·37	II	·18
I_f	·32	II_a	·18
IV	·28	III	·09
VI_{2y}	·28	III_b	·18
V	·25	II_d	·01
II_f	·22	III_a	– ·03

The analysis of Group II in which the correlation is low (r = ·18) shows r = ·18 for II_a, = ·21 for II_b, but when divided into children who had come from elementary schools a year and a half previously (II_c), and children who had come up the school from the Kindergarten (II_d), a marked difference is seen (for II_c = ·32, for II_d = ·01). Here II_c is identical with the median, while II_d gives a result much lower than that for the whole group. On inquiry it was found that the girls forming the II_d part of the group were noted for their apathy about games, and only played under compulsion and were a drag on the group in their play on all occasions. Tested one year later, this group had levelled up to nearer the median result ($r = \cdot 22$).

The analysis for Group III ($r = -\cdot 03$ for III_a, $= \cdot 18$ for III_b) shows that the low correlation is due chiefly to one class.

In Group VI the results obtained with a six months' interval, when practically one third of the school had changed, are almost identical ($r = \cdot 34$ for VI_1, $= \cdot 35$ for VI_2).

The subdivisions for sex show a higher variability amongst the girls ($r = \cdot 42$ for VI_{1y}, $= \cdot 28$ for VI_{2y}). The boys give ($r = \cdot 34$ for VI_{1x}, $= \cdot 33$ for VI_{2x}).

Games Ability v. *Marks.* These results could only be obtained in Groups I and II. In the following table they are compared with the corresponding correlations between Games Ability and the Teachers' Estimate of Intelligence.

TABLE IX.

Groups	Games Ability *v.* Marks	Games Ability *v.* Teachers' Estimate
I	·45	·56
I_c	·52	·6
I_d	·46	·6
II	·21	·18
II_e	·14	·32
II_d	·26	·01
II_f	·21	·22

Results obtained with Tests. Correlation with Play Ability. The highest correlation shown, is that of Games Ability with test A. The results are very uniform in the different groups (for I $r = \cdot 29$, for II $= \cdot 31$, for III $= \cdot 3$, for IV $= \cdot 3$, for V $= \cdot 12$, for VI $= \cdot 3$).

Hardly any correlation seems to exist between Games Ability and the motor test. The e r test shows well-marked correlation in the last three groups (for IV $= \cdot 36$, for V $= \cdot 29$, for VI $= \cdot 25$). Test B shows a correlation in the last three groups ($= \cdot 2$), and a somewhat smaller correlation with the first three groups. Some correlation with Games Ability is found with the Spot Pattern test ($r = \cdot 23$, $-\cdot 2$, $\cdot 14$, $\cdot 29$, $\cdot 14$, $\cdot 18$).

Intelligence v. *Tests.* It is interesting to note that there is a negative or zero correlation between intelligence and the motor test (– ·5, – ·36, – ·1, – ·1). The results for the er test are slightly lower than those of other investigations (see Table II). Intelligence and the B test give a fairly uniform result (r = ·2) while a very slight correlation is found with the Spot Pattern tests.

Correlation with test A is only found in Groups IV and V.

Correlation of the Tests with each other. There is naturally a well-marked correlation between the tests, as they were chosen to test similar qualities. The results from the later groups are probably the most reliable since the conditions under which the tests were made were more uniform and the investigator had had more practice in giving them. The conditions for Group II were rather difficult as it was not possible to get all the children for each of the three tests, and the time at which they were given varied. Hence the results obtained for this group are certainly the least reliable ones.

The motor test gives a high correlation with A as would be expected (I = ·64, III = ·42, IV = ·65, V = ·66, VI = ·9). With the B test it is slightly lower (I = ·27, III = ·42, IV = ·55, V = ·69, VI = ·5). With er test there is a high correlation in the last three groups (IV = ·6, V = ·55, VI = ·63 and ·66). With the Spot Pattern test a high correlation is only found in Group VI.

The er test also correlates highly with the A test (IV = ·5, V = ·63, VI = ·7) and with the B test (IV = ·6, V = ·74, VI = ·7), but not so markedly with the Spot Pattern test (IV = ·45, V = ·33, VI = ·46). Test A naturally shows high correlation with test B (I = ·61, III = ·66, IV = ·72, V = ·6, VI = ·74) and a lower correlation with the Spot Pattern test (·34, ·29, ·34, ·34, ·6).

The reliability coefficients are in each case high. (See Table VII.)

Partial Correlation. Table V. In order to estimate the inter-relation between the different parts of the data obtained, the partial correlation coefficients were worked out where the age, the motor factor, and in certain cases the intellectual factor were eliminated.

1. *Elimination of Age.* The results show that age has no effect on power of doing the tests (Table VI), while the coefficients for Games Ability *v.* Intelligence were slightly raised. (Table IV.)

2. *Elimination of the Motor Factor.* The high correlation of other tests with the motor test leads us to expect that the motor factor involved in this has a strong influence in each case. The partial correlation coefficients were therefore found for Play Ability and the tests

with the motor test constant. The results show that the motor factor plays a large part in the e r test, has no effect on the Spot Pattern test, and only a very slight effect on tests A and B. This seems to show that the motor factor involved in tests A and B differs to some extent from that involved in the motor and e r tests. Larger movements are certainly brought into play in tests A and B, and the higher correlation of test A with Games Ability seems to show that the motor factor in test A more nearly resembles the motor part of a skilled game. By eliminating A for B and Spot Pattern it was possible to show that the A motor factor affected the correlation of both these tests with Play Ability. The results obtained for B and Play Ability, A being constant, were negative in four cases out of the six.

3. The Intelligence factor was eliminated in the same way, and the results show that it makes no difference in the A test, but certainly affects the e r, B, and Spot Pattern tests, a result we should expect, as each of these tests involves a definite intellectual factor.

The General Factor. Table X gives a specimen of the correlation coefficients obtained for the groups separately. The tests are arranged

TABLE X. *Group I. Endowed School* 60 *Girls Age.*

	Test A	G. Int.	B	Games	Motor	Spot P.	e r	Marks
A Test	·6 *·065*	0	·61 *·065*	·29 *·08*	·64 *·05*	·34 *·08*	·13 *·09*	—
G. Int.	0	**·79** (·71) *·03*	·2 *·08*	·56 *·06*	– ·5 *·06*	0	0	·52 *·06*
B Test	·61 *·065*	·2 *·08*	**·43** *·07*	·15 *·09*	·27 *·08*	·32 *·08*	·11 *·09*	—
Games	·29 *·08*	·56 *·06*	·15 *·09*	**·57** *·06*	0	·23 *·08*	0	·45 *·07*
Motor	·64 *·05*	– ·5 *·06*	·27 *·08*	0	—	– ·1 *·09*	·08 =0 *·09*	—
Spot P.	·34 *·08*	0	·32 *·08*	·23 *·08*	– ·1 *·09*	**·55** *·06*	—	—
e r	·13 *·09*	0	·11 *·09*	0	·08 *·09*	—	—	—
Marks	—	·52 *·06*	—	·45 *·07*	—	—	—	—

Figures in italics = probable errors.

Figures in brackets give correlation coefficient found by another method.

Figures in heavy type = reliability coefficient.

22 coefficients > 2 × P.E.

19 coefficients > 3 × P.E.

according to the order of magnitude of the average correlation of each with all the rest (within any particular group), all coefficients smaller than their probable errors being put down as 0. Of the coefficients recorded, the total number of these greater than 2 P.E. is 223, and of those greater than 3 P.E. the total is 188. Similar results were obtained for the other groups. It will be noticed that there is no tendency towards the hierarchical arrangement on which much discussion has recently arisen[1]. These results would appear to agree with those obtained by Brown[2], and differ from those of Spearman, and Wyatt[3]. At the same time owing to the great similarity of the tests used in the present investigation it is impossible to make any assumption from the results obtained, unless the results of such tests are pooled. This has been done in Table XI for Group II, and the hierarchy shown is still so slight that it suggests that the rôle played by a problematic central factor (one common to all the abilities mentioned) is small compared with that played by the specific factors[4].

TABLE XI. *Tests A + B. Motor + e r. Pooled.*

	Spot P.	Int.	A + B	Motor + e r	Age	Games
Spot P.	·90	·30	·70	·61	·33	·18
Int.	·30	·60	·44	·17	0	·35
A + B	·70	·44	·7	·70	·64	·25
Motor + e r	·61	·17	·7	·70	·70	·22
Age	·33	0	·64	·44	—	·14
Games	·18	·35	·25	·22	·14	—

[1] Hart and Spearman. "General Ability, its Existence and Value," this *Journal*, 1912, v. 51—84.

[2] W. Brown. *Op. cit.*

[3] S. Wyatt. *Op. cit.*

[4] The correlation coefficients are rather low. This may be due to the fact that the tests were only taken twice or three times. Brown[5] has shown that to obtain very reliable results tests should be applied a good many times. The writer although cognisant of this fact aimed in this investigation at obtaining a large number of independent results. It was therefore impossible to apply the tests more than two or three times to each group, and the reliability coefficients were sufficiently high to show that the results obtained were reliable.

[5] W. Brown. "The Effects of Observational Errors and Other Factors upon Correlation Coefficients in Psychology," this *Journal*, 1913, Oct.

The higher coefficients in Groups IV, V and VI, as compared with those of the earlier groups, might be due to spurious correlation, produced by taking the whole school as the group instead of special classes as in Groups I, II and III. At the same time the uniformity of the results in the later groups seems to suggest that the high correlation coefficients are probably due to the better conditions under which the results were obtained rather than to the cause suggested above.

5. CONCLUSIONS.

1. A definite correlation ($r = \cdot 32$) exists between Play Ability, as shown in Group Games, and General Ability either as estimated by the teacher or from the form positions for the year.

This correlation is somewhat higher than that which Prof. Karl Pearson found to exist between General Intelligence and Athleticism ($r = \cdot 22$)[1].

2. There is a small positive correlation between Play Ability and age ($r = \cdot 2$). This we should expect to arise from familiarity with the games. It may be also affected by the greater popularity of the group game between certain ages.

3. Social position involving as it does the 'tradition of group games' seems to have little effect on the results, although it is interesting to note that the only group which gives no correlation between Play Ability and General Ability is the group composed entirely of children drawn from elementary schools whose social position is such that the games tradition certainly does not exist.

4. Sex seems to make little if any difference in the correlation between Play Ability and Intelligence. This agrees with Prof. Karl Pearson's results[2].

Correlation between Intelligence or General Ability and Athleticism or Play Ability.

	Boys	Girls	Total
Karl Pearson	·20	·24	·22
M. J. Reaney	·34	·42	·34
	·33	·28	·35

5. Practice seems to have a levelling-up effect, while familiarity with the game for a longer period seems to produce little change in the correlation with intelligence. (See analysis Groups I and II.)

[1] Karl Pearson, *Biometrika*, VII. 542.

[2] Karl Pearson, *Op. cit.*

6. The correlations obtained when the groups are analysed fit in with the results obtained by independent observation and suggest that intelligence goes with good play and lack of interest in games with poor ability. Thus in Group I the class which was noted for games gave a high correlation ($r = \cdot 7$) while the second class which were good average players gave approximately the normal ($r = \cdot 37$). In Group II the lower correlation is seen to be produced by the girls who had been in the school for some time and who were a particularly non-playing set ($r = \cdot 01$), while the girls who had only been one and a half years in the school gave the normal correlation ($r = \cdot 32$).

7. A definite motor factor exists which is common to Play Ability and the various tests. Test A shows this most markedly ($r = \cdot 3$). The correlation of this test with Play Ability is unaffected by Intelligence and is probably entirely due to the motor factor mentioned above. On eliminating the Intelligence factor in the e r, B and Spot Pattern tests the correlations of these with Play Ability are reduced. From this we may assume that Intelligence plays to a certain extent the same part in these tests that it does in 'playing a game.' It is difficult to say what factor is involved. Attention probably plays a very large part and discrimination, power of apprehension, and concentration are made use of. It is of interest to note that there is a definite negative correlation between Intelligence and the motor test. This agrees with Whipple's results for the Cancellation test[1], and fits in with the facts that have been noted by various investigators that the lower intellectual type is better at the simpler motor reactions than the higher type. Of the two purely motor tests I and III, the latter correlates more highly with Games Ability. This is probably due to the fact that it involves larger muscular movements which approximate closely to those employed when playing a game.

8. As results of the investigation, although they are not directly concerned with the objects of the research, the following conclusions may also be mentioned.

(1) That the estimate of the teacher if obtained by amalgamating several independent opinions is very reliable.

(2) That, as far as this investigation affords data, there is little or no tendency towards the hierarchical arrangement; a result which seems to suggest that in this case the specific factors play a more important part than a problematic general factor.

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[1] Whipple, *Op. cit.*

6. Summary.

Very varied opinions have been held as to the relationship existing between General Ability and Play Ability. Some observers regard a high form of intelligence as a necessity for good play while others suggest that an inverse relation exists, the best players often ranking rather low when subjected to intellectual tests.

The results of the investigation described here certainly show that a definite correlation exists between General Ability and Play Ability for Group Games. How far this would hold good if referred to Play under its more diverse and simpler forms needs investigation. The Group Game is the highest form of Motor Play evolved, and it is interesting to note that it is only found in races which have reached a certain stage of mental development. Interesting results would probably be obtained if a more detailed research could be made on the relative correlation of specific games such as Cricket and Football with General Ability.

The results obtained from the analysis of the Groups in this research also point to the fact that marked ability in Games correlates highly with Intelligence, while poor play ability shows a low correlation. This fact is significant especially as the other divisions in each Group give the normal correlation with general ability. It suggests the question: To what extent is the good play the result of Intelligence and the apathy towards play the effect of stupidity, or has the fact of playing the group game keenly for three years definitely improved the Intelligence of the players?—This question cannot be answered until further data have been obtained.

The fact that tradition and social position apparently have very little effect is also interesting, as it seems to contradict the view that the 'games spirit' is found more amongst the class which has had the games tradition for generations. Independent observations on the marked effects of the group game on the social attitude of the child lead us to hope that when given the chance the elementary school child will derive the same benefit from Play that the public schoolboy now does.

The part that Play, in the form of the group game, takes in definitely improving the Intelligence has still to be worked out experimentally, but the remarkable effects which seem to be produced by handwork on brain growth and the results already seen to follow on the establishment of public playing grounds where group games are taught and supervised may well justify us in assuming that this form of Play when properly regulated will come to be regarded as one of the best means of productive education, not only on physical and moral grounds, but on mental grounds also, and that in future no school will exist which does not have the 'organised game' as an important part of its curriculum and not merely as a means of recreation.

I wish to express my sincere and grateful thanks to Dr William Brown for so kindly giving me advice and assistance, to the Head Masters and Head Mistresses of the Schools who granted facilities for the research, and to the assistant masters and mistresses who so ungrudgingly gave their aid.

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THE VALUE OF DISTRIBUTED REPETITIONS IN ROTE LEARNING.

By NELLIE L. PERKINS.

- *I. Introductory.*
- *II. Experimental methods.*
- *III. Experimental data.*
- *IV. Conclusions.*

1. **These** experiments were carried out during the year 1911–12 at the Psychological Laboratory of the University of Michigan, under the direction of Professor Pillsbury. The work of Ebbinghaus[1] had shown the decided advantage of distributing repetitions over several days. The studies of Jost[2] confirmed the result and proved that it was not merely due to the fatigue arising in the case of accumulated repetitions. Dearborn[3] reported some results obtained in class experiments which suggested that practice once a day with a substitution test was somewhat better than practice twice a day. While my own experiments were in progress, Starch[4] published work with a substitution test which indicated that ten minutes at a time twice a day for six days is a better distribution of one hundred and twenty minutes than more condensed arrangements.

Since the first draft of this paper was written, Pyle[5] has published an article in which he distinguishes sharply between length and frequency of periods. He does not, however, sufficiently recognise that the best interval for one length of period may not be the same as the best interval for a different length of period. He used another form of substitution test and concluded that "in habit formation an adult can practice profitably for something like 30 minutes daily." "It is quite probable that in the later stages of habituation, the length of practice period could be shortened and the interval lengthened with practically as much return."

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[1] H. Ebbinghaus, *Über das Gedächtnis*, 1885.

[2] A. Jost, "Die Associations-festigkeit in ihrer Abhängigkeit von der Verteilung der Wiederholungen," *Ztsch. f. Psychol.* xiv. 436.

[3] W. F. Dearborn, "Experiments in Learning," *J. of Educ. Psychol.*, 1910, i. 373—388.

[4] D. Starch, "Periods of Work in Learning," *ibid.*, 1912, iii. 209—213.

[5] W. H. Pyle, "Economical Learning," *ibid.*, 1913, iii. 148—158.

My own experiments were designed to ascertain how much further in the distribution of readings one might go than Jost had gone before a limit would be reached in learning nonsense syllables. The best arrangement of readings involves two questions,—how many repetitions should be used at each period, and, for each number of repetitions per period, what interval between the periods is best.

2. Our apparatus consisted of a modified Wirth memory machine, a Hipp chronoscope, and a voice key. The make-and-break-circuit arrangement was used. The main change in the Wirth apparatus was in the cylinder. A new all-metal cylinder made with moveable contacts was substituted for the original one[1]. The memory apparatus was run either by an electric clock or a metronome, making the response constant. All the experiments were given exactly alike and at the same time of day. Four subjects were used. All of them had had long laboratory experience and were accustomed to giving introspections. They had worked on memory investigations before. They learned with approximately the same ease and, while their methods varied, the per cent. saved in each case ranged about the same. The Scoring (*Treffer*) Method (in a slightly modified form) was the only one used.

Each series for learning was made up of fourteen syllables grouped in pairs. Each set was given sixteen repetitions, either one, two, four, or eight at a sitting. The intervals between sittings were either one, two, three or four days. There were fourteen spaces on the memory disc. A pair of syllables was slipped into every other space, so that syllables and blanks alternated. A three-second exposure was allowed for each space. Thus each syllable was in view three seconds and was followed by a three-second wait. The subject read them without speaking. The order of the syllables in the first reading determined the order throughout the experiment; that is, if the paired syllables were read in the order 1, 2, 3, 4, 5, 6, 7, every succeeding repetition, either for learning or testing, maintained the same order. The only difference between the experiments was the order of distribution of readings for learning. The test occurred two weeks after the last exposure. This is worth emphasis, for much of the work on memory has been done with a

[1] J. F. Shepard and H. M. Fogelsonger, "Inhibition," *Psychol. Rev.*, 1913, xx. 292.

shorter interval between learning and testing, and in many cases it is questionable whether the same laws hold, at least uncomplicated by others, for short and long waits. We have some evidence that divided repetitions are relatively much more effective when tests are made after longer periods than when tested after twenty-four hours. The experiments were carried on over a period of ten months. If often happened that several series fell due on the same day, *e.g.* where a subject was learning one series which was given one repetition every day for sixteen days and another series which was given one repetition every second day. In this case the different series were given with an interval of twenty minutes between them.

The series were not given in a definite order, but each subject was learning at least three series and never more than five series at a time. These were so arranged that there were never more than twelve nor less than six readings a day, *e.g.* a subject might be learning four series—one given two readings a day for eight days, a second given four readings every other day for eight days, a third given two readings every third day for twenty-four days, and a fourth given eight readings every other day for three days—in which case the readings for each day would be as follows:

	1st day	2nd day	3rd day	4th day	5th day, etc.
Series 1	2 readings	2 readings	2 readings	2 readings	2 etc.
Series 2	4 ,,		4 ,,		4 etc.
Series 3	2 ,,			2 ,,	
Series 4		8 ,,		8 ,,	
	8 ,,	10 ,,	6 ,,	12 ,,	

As the readings for each series were completed new series were added according to the above regulation, so that there was continuous learning and testing for ten months—the object being to keep each day as nearly like the preceding days as was possible.

3. Although the subject read the syllables silently in learning, both visual and centrally aroused motor-auditory cues may have played a part in the association process. Accordingly it is sometimes difficult to evaluate an association. One letter may be substituted for another of the same sound in the subject's reply; still the result is correct in an auditory sense. (In making the response the subject was required to

pronounce the syllable and then spell it.) Accordingly for completeness, two calculations of the per cent. correct were made. In the one, the 'auditory recall' of Table I, a reply was given full value if it was correct in sound, *e.g.* *sak* is equivalent to *sac*. If two letters were correct in sound it was given two-thirds value. If only one letter was correct, since this often happens by accident, it was given no credit. In the other calculation, the 'visual recall' of Table I, credit was given only when a letter was exact, regardless of similar sound. A summary of the results is given in the following table, which shows the results for auditory and visual recall.

TABLE I.

	No. of Series	Per cent. correct	Per cent. correct through	
			Auditory Recall	Visual Recall
1 reading every day	10	79	83	74
1 ,, ,, other day	5	72	79	65
1 ,, ,, 3rd ,,	4	82	82	82
1 ,, ,, 4th ,,	4	68	74	56
2 readings ,, day	11	43	54	39
2 ,, other day	8	78	80	74
2 ,, ,, 3rd ,,	6	65	68	56
2 ,, ,, 4th ,,	5	45	53	33
4 ,, ,, day	9	25	28	24
4 ,, ,, other day	10	33	35	25
4 ,, ,, 3rd ,,	11	29	30	23
4 ,, ,, 4th ,,	14	41	47	30
8 ,, ,, day	10	9	14	4
8 ,, ,, other day	13	16	20	10
8 ,, ,, 3rd ,,	10	11	13	5
8 ,, ,, 4th ,,	12	17	24	8

Reading from left to right we see that there were ten instances of sets of seven-paired syllables given one reading a day through sixteen days and tested two weeks after the last exposure. In the test 79 % of the responses were correct both auditorily and visually, meaning by this that the subject pronounced and spelled the syllable as it was given when learned, 83 % were auditorily correct, though they may have been visually incorrect, that is, the subject gave the right sound but spelled the syllables incorrectly, while only 74 % of the responses were visually correct,—which shows that the subject had used motor-auditory cues when learning, although unconsciously—for it is in these terms that the responses were made. There were many instances where the syllables might have been spelled several ways and still have the same sound, *e.g. sac, saq, sak.* In these cases it often happened that the subject could give the correct sound, but had forgotten how it was spelled.

The results for the different subjects are shown in Tables II—V. Each table shows the number of cases, the average reaction times, and the per cent. correct for each variety of test. A part of the reaction times was not obtained with Pk., and several varieties of tests were omitted with W. and M. This is indicated by dashes.

TABLE II. *Subject Pk.*

1	2	3	4	5	6	7
	No. of Series	Per cent. correct	Average reaction time*	Mean variation	Average reaction time for wrong response	Average reaction time for right response
1 reading every day	5	71	4″·727	2″·524	5″·094	4″·846
1 ,, ,, other day	2	73	4″·033	1″·590	6″·337	3″·229
1 ,, ,, 3rd ,,	2	64	6″·208	5″·001	5″·371	3″·103
1 ,, ,, 4th ,,	3	67	3″·202	1″·462	5″·367	2″·537
2 readings ,, day	3	53	4″·519	2″·863	6″·460	3″·135
2 ,, ,, other day	3	67	4″·833	2″·956	7″·364	7″·452
2 ,, ,, 3rd ,,	3	66	4″·529	3″·271	8″·565	2″·433
2 ,, ,, 4th ,,	3	47	5″·165	1″·321	5″·641	4″·691
4 ,, ,, day	3	29	3″·269	2″·160	2″·571	4″·191
4 ,, ,, other day	3	37	—	—	—	—
4 ,, ,, 3rd ,,	3	28	—	—	—	—
4 ,, ,, 4th ,,	3	69	—	—	—	—
8 ,, ,, day	3	6	—	—	—	—
8 ,, ,, other day	3	14	—	—	—	—
8 ,, ,, 3rd ,,	3	9	—	—	—	—
8 ,, ,, 4th ,,	5	20	—	—	—	—

* The average reaction time was computed by finding the average time for all responses, which in this case include both right and wrong responses and what in the discussion is called 'giving up,' that is, when the subject could not recall the syllable, and the response was 'don't know,' the time always being recorded just as if he had given a syllable. As there is no discussion of the reaction time for 'giving up,' it is not included in the tables except in so far as it has influenced the time given in Columns 4 headed Average Reaction Time. This accounts for the difference between the average reaction time given in the tables and that which takes into consideration only the average time for right and wrong responses.

In nearly all cases with each subject the reaction time for a wrong response will be found to be longer than that for a correct response. (Compare Columns 6 and 7 in the tables.) In most results there was a feeling of certainty with the correct responses and they were given at once, whereas there was dissatisfaction when the wrong response was given which made the subject hesitate before she responded.

TABLE III. *Subject A.*

1	2	3	4	5	6	7
	No. of Series	Per cent. correct	Average reaction time	Mean variation	Average reaction time for wrong response	Average reaction time for right response
1 reading every day	1	95	1″·799	0″·371	—	1″·799
1 ,, ,, other day	2	57	2″·501	1″·761	6″·873	1″·392
1 ,, ,, 3rd ,,	1	100	0″·998	0″·392	—	0″·998
1 ,, ,, 4th ,,	1	71	1″·856	0″·751	2″·741	1″·501
2 readings ,, day	2	7	1″·783	0″·815	1″·272	0″·926
2 ,, ,, other day	1	71	2″·346	1″·117	3″·910	1″·721
2 ,, ,, 3rd ,,	2	73	1″·603	0″·679	3″·976	1″·208
2 ,, ,, 4th ,,	1	42	—	—	—	—
4 ,, ,, day	1	42	4″·442	4″·225	8″·128	1″·982
4 ,, ,, other day	2	0	3″·918	1″·817	3″·918	—
4 ,, ,, 3rd ,,	2	14	2″·779	0″·877	2″·761	2″·993
4 ,, ,, 4th ,,	2	37	3″·240	—	5″·141	2″·289
8 ,, ,, day	1	0	4″·985	1″·228	4″·985	—
8 ,, ,, other day	1	28	6″·021	2″·440	6″·444	4″·961
8 ,, ,, 3rd ,,	1	23	2″·143	0″·846	—	2″·143
8 ,, ,, 4th ,,	1	0	5″·262	1″·701	5″·262	—

Reading from left to right—as in Table I—we see that subject Pk. learned five sets of seven-paired syllables given one reading every day through sixteen repetitions and was tested two weeks after the last exposure. In the test she recalled 71 % of the syllables, the average reaction time being 4·727 seconds with a mean variation of 2·524 seconds. The results for each set may be followed in the same way, and comparisons made by running down the percentages correct for each set in Column 3.

Table VI gives the summary of all results and is to be read from left to right in the same way as the preceding tables were read.

In general, it will be observed that less is recalled when the readings are given every day than when a certain interval, varying from one to

TABLE IV. *Subject W.*

1	2	3	4	5	6	7
	No. of Series	Per cent. correct	Average reaction time	Mean variation	Average reaction time for wrong response	Average reaction time for right response
1 reading every day	2	78	4″·345	2″·596	5″·071	4″·200
2 readings ,, ,,	1	57	6″·258	3″·851	1″·031	3″·219
2 ,, ,, other day	1	85	1″·216	0″·226	1″·326	1″·198
4 ,, ,, day	3	18	7″·161	3″·267	8″·146	2″·405
4 ,, ,, other day	5	43	4″·221	2″·184	6″·514	2″·035
4 ,, ,, 3rd ,,	4	33	5″·898	3″·806	7″·418	2″·690
4 ,, ,, 4th ,,	3	42	4″·146	1″·703	6″·379	2″·564
8 ,, ,, day	2	26	5″·899	1″·897	1″·252	5″·323
8 ,, ,, other day	6	15	5″·300	2″·014	5″·770	2″·170
8 ,, ,, 3rd ,,	6	10	5″·426	2″·012	5″·524	5″·262
8 ,, ,, 4th ,,	6	18	5″·518	2″·699	5″·909	4″·388

TABLE V. *Subject M.*

1	2	3	4	5	6	7
	No. of Series	Per cent. correct	Average reaction time	Mean variation	Average reaction time for wrong response	Average reaction time for right response
1 reading every day	2	92	2″·209	1″·875	1″·601	2″·360
1 ,, ,, other day	1	100	2″·480	1″·063	—	2″·480
1 ,, ,, 3rd ,,	1	100	1″·437	0″·221	—	1″·437
1 ,, ,, 4th ,,	—	—	—	—	—	—
2 readings ,, day	5	60	4″·475	2″·681	3″·762	4″·247
2 ,, ,, other day	3	88	3″·938	4″·271	1″·281	4″·743
2 ,, ,, 3rd ,,	1	47	4″·898	3″·125	2″·827	5″·585
2 ,, ,, 4th ,,	1	42	3″·608	1″ 595	3″·154	3″·949
4 ,, ,, day	2	21	4″·631	1″·774	5″·270	2″·875
4 ,, ,, other day	—	—	—	—	—	—
4 ,, ,, 3rd ,,	2	33	5″·119	4″·002	6″·646	3″·581
4 ,, ,, 4th ,,	6	29	5″·598	3″·811	5″·687	5″·147
8 ,, ,, day	4	7	5″·265	2″·238	5″·366	4″·762
8 ,, ,, other day	3	17	4″·252	2″·439	5″·205	1″·634
8 ,, ,, 3rd ,,	—	—	—	—	—	—
8 ,, ,, 4th ,,	—	—	—	—	—	—

TABLE VI.

1	2	3	4	5	6	7
	No. of Series	Per cent. correct	Average reaction time	Mean variation	Average reaction time for wrong response	Average reaction time for right response
1 reading every day	10	79	3″·854	2″·193	4″·588	3″·915
1 ,, ,, other day	5	72	3″·109	1″·553	6″·605	2″·344
1 ,, ,, 3rd ,,	4	82	3″·713	1″·969	5″·376	2″·160
1 ,, ,, 4th ,,	4	68	2″·865	2″·498	4″·710	2″·278
2 readings ,, day	11	43	4″·160	2″·852	4″·728	3″·478
2 ,, ,, other day	8	78	3″·435	2″·723	3″·470	3″·100
2 ,, ,, 3rd ,,	6	65	4″·018	1″·389	6″·500	2″·818
2 ,, ,, 4th ,,	5	45	4″·776	2″·744	5″·019	4″·505
4 ,, ,, day	9	25	5″·754	2″·079	6″·149	2″·788
4 ,, ,, other day	10	33	4″·135	3″·123	6″·061	2″·035
4 ,, ,, 3rd ,,	11	29	4″·923	3″·108	5″·840	2″·988
4 ,, ,, 4th ,,	14	41	4″·927	2″·003	7″·101	3″·968
8 ,, ,, day	10	9	5″·830	2″·103	5″·731	5″·048
8 ,, ,, other day	13	16	5″·176	1″·792	5″·364	3″·036
8 ,, ,, 3rd ,,	10	11	4″·840	2″·342	5″·507	3″·933
8 ,, ,, 4th ,,	12	17	5″·212	—	—	4″·457

three days, is allowed to elapse between the readings. This ability to remember more after the lapse of a few days than immediately after learning has been termed 'reminiscence' by Ballard, who has shown that it is more active among old and consolidated associations than among those recently established, and that it is accompanied by an increase in the facility (speed) of reproduction[1].

4. Since Jost had shown the advantage of distribution up to two repetitions a day, we devoted ourselves to two main problems: (1) to discover if there was a limit to the degree of distribution that might be advantageous, and (2) to determine, in cases where more than one repetition was made on a single day, if learning was easier when more than one day was allowed to intervene between the groups of readings, and to discover, if possible, the most favourable interval.

The tables indicate that the highest percentage of success is obtained by the use of one reading at each sitting (see Table VI, Column 3), and, in general, the smaller the number of repetitions used at a sitting, the higher the percentage of success. As to the interval between sittings, the most favourable interval seems to be between two and three days for cases in which either one or two repetitions are used at a sitting; for cases in which four or eight repetitions are used at a sitting, the longer intervals of three or four days have a value equal to or greater than the shorter intervals. There are exceptions to these statements, particularly with subject A., with whom few experiments were made. But a larger number of results is necessary to determine the best interval accurately. So far as the tables show, the reaction time does not vary altogether consistently with either of the factors concerned, although the more divided readings give the shortest times in general, as do the moderate intervals.

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[1] P. B. Ballard, "Obliviscence and Reminiscence," *Monogr. Suppl. II to this Journal.*

If one accepts the law of Jost that the advantage of distributed learnings is due to the greater effectiveness of old associates for relearning, our results indicate that the process of 'consolidation' continues for at least forty-eight hours and still longer if four or more readings are made on each day. The results of permitting the hypothetical consolidation process to take place are strikingly shown by the fact that with one reading a day for sixteen days the retention after two weeks was approximately 80 %, while Ebbinghaus found that with accumulated repetitions sixteen readings just sufficed to learn a series of twelve syllables, so that they could be said through once without mistake.

(*Manuscript received* 14 *June* 1914.)

PUBLICATIONS RECENTLY RECEIVED.

Grundzüge der Psychotechnik. Von Professor HUGO MÜNSTERBERG. Leipzig: Barth. 1914. S. xii + 767. M. 16; geb. M. 17.

Psychotechnik is the name Prof. Münsterberg gives to one of the two halves into which he divides applied psychology; the other half he terms *Kulturpsychologie.* Just as mathematics may be applied, on the one hand, to the engineering of bridges and tunnels, or, on the other hand, to the more abstract problems of pure physics, so, he points out, may psychology be applied either to practical problems of the present (*Psychotechnik*), showing us how to act, or to theoretical problems concerning what has happened in the past. This book seeks for the first time to determine the fundamental principles of the new science, to survey from a common standpoint, and as an organic whole, its various branches—the *Psychotechnik* of sociology, therapeutics and hygiene, industrial efficiency, law, education, art and science—and to indicate the future scope of the subject. Those who have read the series of books on the applications of psychology which have of late years come from Prof. Münsterberg's pen, will understand his desire and realise his fitness to deal with the subject in its entirety. The book, however, is not to be regarded as an epitome of our knowledge of the subject. Even within its wide limits space can only be found for mention of the more important or more striking experiments in each branch. The author's main interest clearly lies in the more general abstract treatment of the problems at issue, and this is carried out with a patience and thoroughness that come only from an unusual pre-occupation in the principles that underlie human knowledge and progress.

On Dreams. By Professor S. FREUD. Translated from the second German edition by Dr M. D. EDER, with an Introduction by Dr W. LESLIE MACKENZIE. London: Heineman. N.D. Pp. xxxii + 110. 3*s.* 6*d.* net.

This attractive little book is to be regarded as a popular introduction to the author's larger *Traumdeutung*, to which it is indeed superior in its more moderate and more succinct presentation of his views; the translator, too, has approached a satisfactory English rendering much more closely.

The Unconscious: the fundamentals of human personality normal and abnormal. By Dr MORTON PRINCE. New York: The Macmillan Co. 1914. Pp. xvi + 549. 8*s.* 6*d.* net.

The substance of this book, here given in the form of (sixteen) lectures, has already appeared in a series of papers in the *Journal of Abnormal Psychology.* It is based on the assumption that the "field of conscious states" contains (*a*) an inner focus of attention surrounded by (*b*) a marginal area of attention, external to which is (*c*) an area of co-conscious ideas "not entering into conscious awareness," beyond which again lies (*d*) the region of unconscious processes comprising (i) conserved, dormant neural dispositions (the physiological basis of memory) and (ii) active neural (*e.g.* spinal) processes. According to Dr Prince, (*c*) and (*d*) are divisions of the subconscious. Elsewhere, he regards perception as "made up of a primary sensory image of an object combined with a number of secondary images" (p. 327), and the difference between perception and meaning as being "one of complexity. The less complex we call perception, the more complex, meaning" (p. 328). "The 'unity of consciousness,'" he concludes, "seems to be a cant-expression uttered by some unsophisticated ancient philosopher and repeated like an article of faith by each successive generation....Neither a reference to the evidence of consciousness or to its manifestations gives support to the notion of unity. The mind is rather an aggregation of potential or functioning activities some of which may combine into associative functioning processes at one time and some at another; while again these different activities may become disaggregated with resulting contraction of personality, on the one hand, and conflicting multiple activities on the other" (p. 538). Independently of the extent to which such views meet with the acceptance of psychologists, the merit of Dr Prince's book undoubtedly lies in the wealth of observation, derived from his extensive clinical experience, which he introduces in illustration.

Le Sommeil Automatique: contribution à la pathologie du sommeil. Par le Dr GEORGES POYER. Paris: Leclerc. 1914. Pp. 93.

This interesting little monograph consists of three chapters devoted to automatic action, sleep and volition, and automatic sleep respectively. In the first of these, after a (too narrow) historical survey of the notion of automatism, the author aims at a classification of the various "phenomena" in question. In the second he gives a suggestive psychological analysis of the act of going to sleep, showing how in the normal process we can often detect (i) feelings of fatigue and the need for sleep, (ii) the idea of sleeping, and (iii) the will to sleep (*volition hypnique*)—a truly volitional act, resembling the will not to attend, and accompanied by a lowering of muscular tone, a state of mental dissociation, and often by a feeling of effort. In the last chapter he comes to his main subject, citing in illustration eight cases (all suffering from delusions of persecution) from among the inmates of the Maison Nationale at Charenton. These patients complained that frequently they did not go to sleep of their own will, but were forced into an artificial sleep by others owing to the influence of magnetic currents, electrical fluids, irresistible pressure on the eyelids, etc. To this kind of sleep the author gives the name 'automatic,' on the ground that it can be best explained by an automatization of processes, normally under partial voluntary control, which occur in going to sleep, especially muscular relaxation and the "raising of the threshold" for complex reactions.

Know your own Mind. By W. GLOVER. Cambridge: University Press. 1914. Pp. x + 204. 2*s*. net.

A most stimulating little book, written from the Herbartian standpoint and intended for popular reading.

The Anatomy of the Brain. By Professor J. F. BURKHOLDER. With an Introduction by Professor H. H. DONALDSON. Chicago: Engelhard & Co. 1912. Pp. 206. $2.00.

A number of new illustrations has been included in the second edition of this work, with the addition of much new matter in the text. The book has been written in order to assist the student in the dissection of the sheep's brain, which, the author claims, makes an exceedingly good substitute in the dissecting room for the human brain.

Le Problème Mondial: études de politique internationale. By Dr ALBERTO TORRES. Rio de Janeiro: Imprensa Nacional. 1913. Pp. xviii + 213.

Julius Robert Mayer: seine Krankheitsgeschichte und die Geschichte seiner Entdeckung. Von Dr ERNST JENTSCH. Berlin: Julius Springer. 1914. Pp. viii + 135. M. 4; geb. M. 4.80.

Zum gegenwärtigen Stand der Erkenntnistheorie. Von Professor THEODOR ZIEHEN. Wiesbaden: Bergmann. 1914. S. 73. M. 2.80.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY.

July 4, 1914[1]. The *Rôle* of Repression in Forgetting, by T. H. PEAR, T. W. MITCHELL, A. WOLF, and T. LOVEDAY.
On Freedom, by S. ALEXANDER.

July 5, 1914[1]. The Status of Sense Data, by G. E. MOORE, G. F. STOUT, and G. DAWES HICKS.

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[1] In conjunction with the Aristotelian Society and the *Mind* Association.

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VOLUME VII OCTOBER, 1914 PART 3

THE MEASUREMENT OF INTELLIGENCE IN CHILDREN BY THE BINET-SIMON SCALE.

BY AGNES L. ROGERS AND J. L. McINTYRE.

I. The principles underlying the scale.
II. The form of the scale.
III. Methods and results:
  (1) *Distribution in mental and physical age, for the* 1908 *and* 1911 *scales.*
  (2) *Correlation between teacher's ranking and scale results.*
  (3) *Sex differences.*
  (4) *Comparison of distribution of ability in French, German, American and Scottish children.*
IV. The question of standard.
V. Suitability of the tests:
  (1) *Tests in which Aberdeen children show backwardness or precocity: the definition test.*
  (2) *Tests depending on school training.*
  (3) *Tests depending on home surroundings.*
VI. Conclusions as to the choice of test and form of scale.

I. THE PRINCIPLES UNDERLYING THE SCALE.

THE need of a more scientific method for discriminating between normal and abnormal children and for diagnosing and classifying the abnormal, has roused exceptional interest in the Binet-Simon Scale. Within the last few years numerous studies have been issued both in America and in Europe in commendation, criticism, revision, and extension[1]. Too often these have ignored the particular purpose that its authors had in view when they devised their new method for measuring intelligence. A commission appointed by the French

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[1] *Vide* W. Stern, *Die psychologischen Methoden der Intelligenzprüfungen*, Leipzig, 1912 (with a bibliography): E. Meumann, *Arch. f. d. ges. Psychol.* 1912: J. Carleton Bell, "Recent Literature on the Binet Tests," *J. of Educ. Psychol.* Feb. 1912.

Minister for Education in 1904 had recommended that no child should be transferred to a Special School before undergoing an examination both pedagogical and medical. The existing methods of investigating cases on the border line between normality and abnormality were obviously inadequate, and the publication of the Binet-Simon Scale in 1905 was an attempt to meet this practical situation. It marked a stage in the treatment of the problem of intelligence by measurement; it was designed to take the place of mere subjective impressions in the diagnosis of mental deficiency, by supplying an objective standard based on the relation of intellectual capacity to age. It was never intended to make a qualitative examination of the intellectual powers, but only to determine quantitatively the level of mental ability attained. The theoretical problems of the analysis of intelligence into its constituent factors, their inter-relation, and the nature of their separate contributions to the mentality as a whole, are entirely distinct. No doubt the application of the scale may furnish material for the analysis of intelligence, and the analysis in turn may lead to the gradual improvement of the scale, but the two aims demand different procedures. An essential element in analysis, introspection, is impossible under the conditions of the method of measurement by age-grades.

The theory of intelligence underlying this method is essentially functional. Intelligence is regarded as the complex resultant of a number of capacities by which a person adapts himself to his environment, physical and social. It is the power to use experience to advantage, and may be divided roughly into the ability to learn and the ability to apply what has been learned. Thus intelligence is not a single, or abstract, or central function. There is no general intelligence distinct from specific abilities. The name denotes a group of relatively independent capacities assumed to be present in some measure in every healthy child, and the extent to which these powers have been exercised, as well as the kind of material upon which they have been exercised, determines the degree of intelligence attained[1].

It follows that no single test will suffice for the measurement of intelligence. The cardinal principle of procedure is to apply a number of tests so varied in nature as to embrace all the important intellectual functions; only so may individual differences be allowed for and incidental errors eliminated. Binet and Simon had no definite scheme of abilities in mind when they devised the scale. Their guiding idea was to select a large number of tests of varying difficulty, which were afterwards graded on the basis of the experimental results obtained by their use. The factors in intelligence which they held to be most important were *comprehension, invention, direction* and *censure*, which we may roughly translate as the abilities to grasp or understand, to plan or devise, to organise or administer, and to judge or criticize. These are the functional units of intelligence, just as memory, association, attention and the like are its structural units. If we use the more familiar structural analysis of mind, the scale is seen to cover a variety of formal functions. There are tests of the span of apprehension, and of sensory discrimination; the power to distinguish and compare is gauged not only as regards presented material, but also with reference to objects, concrete and abstract, which are not presented. There are several tests involving imagery, both visual and auditive; while reproduction, graphic and oral, immediate and remote; association, free and controlled; and the abilities to describe and report, are each tested both for accuracy and for range or speed. Several tests are designed to measure imaginative, inventive, and constructive ability, both mechanical and linguistic; others appreciate the capacity for judgment, aesthetic and practical, the aptitude for reasoning, arithmetical and logical, and the control of attention. A test of suggestibility, included in the 1905 scale, but omitted in 1908, was reintroduced in the revised scale of 1911. Thus the variety of mental processes called into play by the scale as a whole is comprehensive.

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[1] Binet and Simon, *L'Année Psychol.* 1909, 183, and Binet, *Les idées modernes sur les enfants*, Paris, 1911, 117 and 242 ff.

The scale has also been designed to touch as many of the child's natural interests as possible, and accordingly many of the tests deal with practical problems. Some refer to matters of universal interest such as time-reckoning, direction in space, and the use of money. Others imply a knowledge of social or of domestic affairs. Only a few depend upon special instruction acquired in school: these were first introduced in 1908 and ultimately rejected in 1911. On the whole, the scale makes a wide appeal, and succeeds to a remarkable extent in revealing the grasp a child has of his environment.

II. THE FORM OF THE SCALE.

In form, the scale has undergone two important changes. When it was first published in 1905, the diagnosis and classification of the abnormal was the object in view, and for this purpose a series of 30 tests was selected, with regard to two main principles. In the first place, the method of investigation was to be strictly psychological; pure intelligence alone was to be measured, and consequently the influence of training in home or school was as far as possible to be eliminated. The pedagogical examination recommended by the 1904 Commission would thus necessitate a separate scale, designed to measure the degree of scholastic attainment. Binet supplies such a scale in *Les idées modernes sur les enfants*, p. 25 ff. In the second place, the level of pure intelligence was to be objectively determined on the basis of the relation of intellectual capacity to age. Having been applied to normal children and their relative difficulty determined, the 30 tests were arranged in a series, so that the first corresponded to the lowest level of intelligence and the last to the level of normal adult capacity. They were not assigned to definite ages, because all that was desired at this time was a rough classification into the grades of mental retardation. Certain points in the scale marked the limits of these grades. Thus the first seven tests could be successfully passed by normal children of 3, the first fifteen by children of 7, the first twenty-two by children of 9, and the remaining tests by children of 11 and upwards; and correspondingly, 'idiocy' was indicated where an adult succeeded in passing the first seven alone, 'imbecility' where there was success in only the first fifteen, 'feeble-mindedness' by success in the first twenty-two, and failure beyond that point.

An instrument capable of diagnosing and classifying mental insufficiency was thus provided, which was soon put to the proof by other investigators and subjected to some criticism. Decroly and Degand[1] suggested improvements in the choice of tests and in their order of arrangement. Their own increasing experience in the use of the scale convinced Binet and Simon that its real weakness lay in the fact that there was no standard of normal ability for each age. They decided to undertake the task of providing such a standard, and accordingly a new scale was published in 1908, which embodied this idea of allotting the tests to definite ages. Another important departure in principle was made at the same time. The strictly psychological method of 1905 was now replaced by a combination or blend of the psychological and pedagogical methods of intelligence-measurement. Thirty new tests were introduced, and these were for the most part designed to determine the degree of *acquired capacity*, or range of information, rather than innate ability. A few of the original tests were discarded, and the remaining fifty-six were distributed over ages 3 to 13, the number varying at the different ages, in some four, in others five, and in one case as many as eight.

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[1] Decroly et Degand, *Arch. de Psychol.* 1906, VII.

As a result of further experience and criticism, the scale was again revised in 1911. The principle underlying the 1905 scale, that a strictly psychological method of measurement should be used, was once more inforced. Consequently several tests in the 1908 scale, depending upon training in school or home, were rejected. New tests were also added, and the grading of tests was carefully reconsidered. The chief advance, however, was the allotment of the same number of tests to each age, as it made possible a more accurate estimation of the degree of mental development attained. It is of fundamental importance that the method of calculating the level of intelligence from the results of the application of the separate tests should be reliable, since the *crux* of the whole problem of the measurement of mental efficiency by age-grades is the determination of a satisfactory standard. This arises not only with regard to each individual test, but also with regard to the assigning of a 'mental age' to each child on the basis of its successes or failures; and it arises once again in estimating the reliability of the whole scale.

The satisfactoriness of the *individual* test was determined experimentally by Binet and Simon from the investigation of 203 carefully selected normal French children. It was assumed that a test was suitable for that age at which a large majority of children gave a correct response[1]. Apparently Binet did not recognise the need for a *definite* standard in assigning the tests to the various ages. In the article in the *Année psychologique* published in 1908, such statements as he makes in this connexion are extremely vague, *e.g.* "The test is passed by a majority of the children of this age," "Almost all the children pass this test," etc. These sentences reveal in typical fashion how the authors of the scale were only 'feeling their way' towards a reliable instrument for measurement. The tests having thus been assigned to the various years, the question arose as to how the *mental age* of a given child was to be determined. In 1908 Binet and Simon adopted the method of giving a child as its mental age the highest year, of which it passed all the tests, or all but one, adding one year for every five additional tests in which it succeeded. In 1911, on account of the improvements made in the scale, they proposed to mark a child to the highest age at which he passed all five tests, adding one-fifth of a year for each additional test beyond that point. The consideration of the sufficiency of these standards, as well as of the reliability of the scale as a whole, may be postponed until we have given the results of our application of the tests.

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[1] See Bobertag, *Ztsch. f. angew. Psychol.* VI. 504.

III. METHODS AND RESULTS.

Our investigation was carried out in the Demonstration School attached to the Aberdeen Training Centre. The tests were carried out by the writers of this paper and by Miss Jean S. Templeton. We examined altogether 217 children ranging in age from 4 to 14, applying all the tests required by the 1908 and 1911 scales. The numbers at each age are shown in Table I. For the most part, the children in this school came from working class homes. The tests were invariably given between 10 and 12 in the forenoon, and each subject was taken individually. The examination in general took 40 to 45 minutes[1]. The Binet-Simon procedure was adhered to as strictly as possible, as we soon became aware that the results could be affected by very slight differences in method. Where it was necessary to make any change, we adopted for the most part the suggestions of Whipple in his *Manual of Mental and Physical Tests.* In several instances, however, other versions were used, in order to have tests equal in interest or difficulty, with Binet's. These alterations are as follow:

TABLE I.

Age	Number
4	5
5	21
6	23
7	21
8	22
9	24
10	24
11	37
12	20
13	18
14	2
Total	217

Tests 15 and 52 (*Memory for Spoken Phrases*), 44 (*Problem Questions*) and 45 (*Trick Phrases*). In these questions we made such alterations as were necessary for Scottish conditions, *e.g.* 'Engine driver' for 'Engineer,' 'carriages' for 'wagons' and the like. In test 44, we made use of several other questions from the 1905 scale as well as those adopted by Binet in 1908, since it seemed necessary to vary the problems in the case of such small classes as those with which we were dealing. Problem No. 15 was not applied, as it was considered unsuitable. We agree with Meumann and Ayres that even adults would find difficulty in answering some of these questions.

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[1] We would endorse Binet's concluding remarks in *L'étude expérimentale*, etc. (p. 299), on the dangers that attend haste in such investigations.

Tests 17 and 38 (*Definitions of Familiar Objects*). The words we gave to be defined were (1) *fork*, (2) *table*, (3) *chair*, (4) *horse*, (5) *school*. Tests 23 (*Writing from Copy*), 43 and 46 (*Sentence Building*). In these tests the children used pencil instead of ink, to which the younger ones were not accustomed.

Test 26 (*Description of a Picture*). The pictures used were scenes of out-door life and were probably easier to interpret than those of Binet.

Tests 28 and 42 (*Naming Coins*); the coins used were ½*d.*, 1*d.*, 6*d.*, 1*s.*, 2*s.*, 2*s.* 6*d.*, 5*s.*, 10*s.*, 20*s.* respectively.

Test 30 (*Counting Money*) 2 halfpennies and two sixpences were used. Test 37 (*Giving Change*), a florin was given in payment for an article costing 4*d.*, the child returning the correct change for a 'pass'. He had at his disposal two halfpennies, 13 pennies, 5 sixpences, 2 shillings, 1 florin, 1 crown, 1 half-sovereign, and 1 sovereign.

Tests 29 and 39 (*Reading and Report*). A similar passage to that of Binet was given, adapted to local conditions.

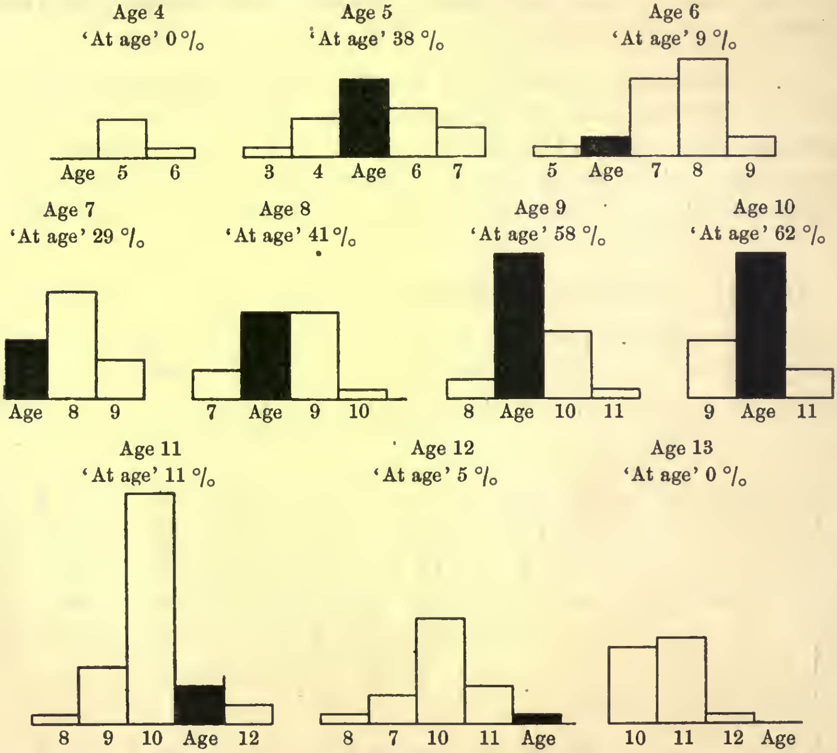
1. *Distribution in Mental and Physical Age, for the* 1908 *and* 1911 *Scales.* The results of our application of the 1908 and 1911 scales are given in Tables II and III, and in Figures 1 to 4. We may assume that about 50 °/₀ of the children of a given physical age should be of the same mental age. With the earlier scale, only ages 5, 8, 9 and 10

TABLE II. *Distribution in Mental and Physical Age.* 1908 *Scale.*

Phys. Age	Mental Age												Total	'At age' °/₀	Median Test Age
	3	4	5	6	7	8	9	10	11	12	13	14			
4	—	**0**	4	1	—	—	—	—	—	—	—	—	5	0	5·6
5	1	4	**8**	5	3	—	—	—	—	—	—	—	21	38	5·6
6	—	—	1	**2**	8	10	2	—	—	—	—	—	23	8·7	8·0
7	—	—	—	—	**6**	11	4	—	—	—	—	—	21	28·6	8·4
8	—	—	—	—	3	**9**	9	1	—	—	—	—	22	40·9	8·8
9	—	—	—	—	—	2	**14**	7	1	—	—	—	24	58·3	9·6
10	—	—	—	—	—	—	6	**15**	3	—	—	—	24	62·5	10·3
11	—	—	—	—	—	1	6	24	**4**	2	—	—	37	10·8	10·4
12	—	—	—	—	—	1	3	11	4	**1**	—	—	20	5	10·5
13	—	—	—	—	—	—	—	8	9	1	**0**	—	18	0	11·1
14	—	—	—	—	—	—	—	2	0	0	0	**0**	2	0	—
	1	4	13	8	20	34	44	68	21	4	0	0	217		

TABLE III. *Distribution in Mental and Physical Age.* 1911 *Scale.*

Phys. Age	Mental Age 3	4	5	6	7	8	9	10	11	12	13	14	Total	'At age' %	Median Test Age
4	—	**1**	3	1	—	—	—	—	—	—	—	—	5	20	5·4
5	1	5	**11**	3	1	—	—	—	—	—	—	—	21	52·3	5·4
6	—	—	3	**4**	11	5	—	—	—	—	—	—	23	17·3	7·4
7	—	—	—	3	**8**	9	1	—	—	—	—	—	21	38	7·9
8	—	—	—	1	7	**10**	3	1	—	—	—	—	22	45·4	8·2
9	—	—	—	—	—	10	**13**	1	—	—	—	—	24	54·1	9·1
10	—	—	—	—	—	1	11	**9**	1	2	—	—	24	37·5	9·9
11	—	—	—	—	—	4	9	16	**5**	1	1	1	37	13·5	10·3
12	—	—	—	—	—	1	6	4	6	**2**	1	—	20	10	10·8
13	—	—	—	—	—	—	4	3	4	5	**2**	—	18	11	11·5
14	—	—	—	—	—	—	—	2	0	0	0	**0**	2	0	—
	1	6	17	12	27	40	47	36	16	10	4	1	217		


Age 4
'At age' 0 %

Age 5
'At age' 38 %

Age 6
'At age' 9 %

Age 7
'At age' 29 %

Age 8
'At age' 41 %

Age 9
'At age' 58 %

Age 10
'At age' 62 %

Age 11
'At age' 11 %

Age 12
'At age' 5 %

Age 13
'At age' 0 %

Fig. 1. 1908 Scale. Distribution of Test Ages.

approximate to this; by the later scale, 5, 7, 8, 9 and 10 conform more or less adequately. From another point of view, a slight superiority in the revised scale is shown by the comparison of the distribution of abilities for all ages combined. According to Goddard's method of grouping, children 'at age,' or one year above or below, are regarded as *normal,* those who are two or more years above their physical age are accounted *gifted,* while those whose mental age is two or more years behind their physical age are *backward.* On the 1908 scale, 70½ °/₀ are normal, 19½ °/₀ backward, and 10 °/₀ gifted, while, with the 1911 scale, 75½ °/₀ are normal, 18½ °/₀ backward, and 6 °/₀ gifted. The latter numbers approximate more closely to the 'ideal' distribution (*vide infra*). In spite of its numerical superiority in normal cases, the frequency curve

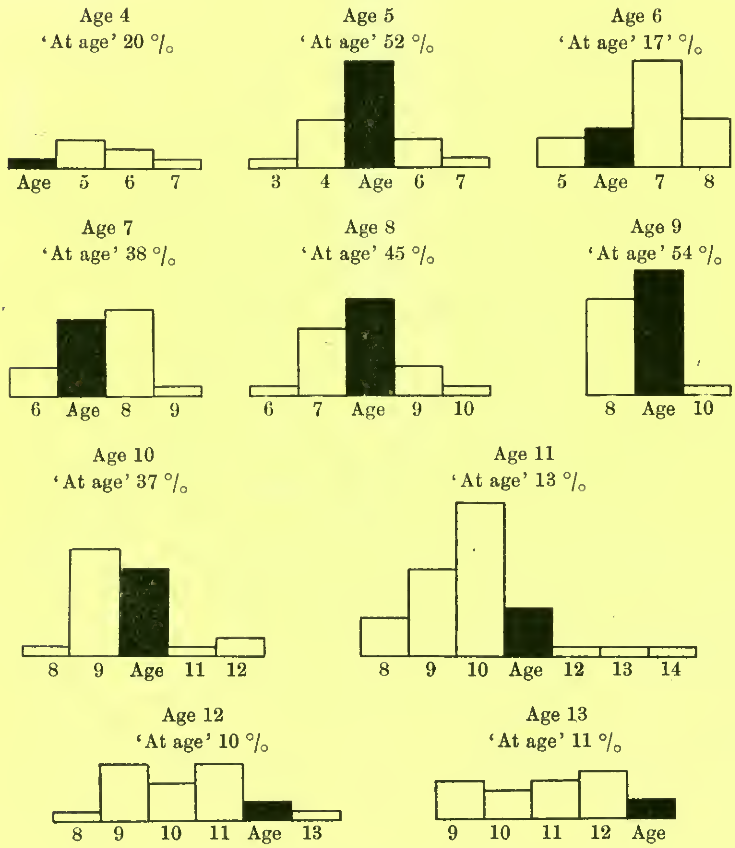


Fig. 2. 1911 Scale. Distribution of Test Ages.

of the 1911 scale, being less symmetrical, is less satisfactory than that of 1908. Our results also corroborate the view that both scales are too easy for the earlier years, and too hard for the later.

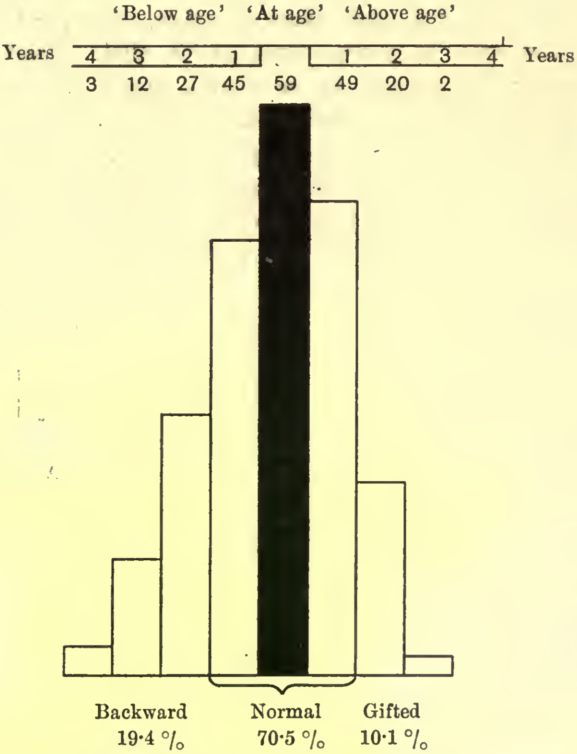


Fig. 3. 1908 Scale. Distribution in Mental and Physical Age.

2. *Correlation between Teacher's ranking and Scale results.* The correlation indices between the teacher's ranking of their pupils and our ranking by the 1911 scale are disappointing. The teachers were asked to arrange the pupils, boys and girls together, in order of merit, as determined by the class work and general intelligence: the results are shown in Table IV (the Spearman 'foot-rule' formula being used).

On the average, the index obtained is over 0·5. In one or two classes, however, it is very low, and in one only is it comparatively high. The probable errors show that with the exception of the 7th and the 8th grades, all the correlation-coefficients are sufficiently reliable, being, with these exceptions, more than three times the probable error.

The variations from class to class, as well as the *comparative* lowness of the average index, may be referred to a variety of causes: (1) Whereas the teacher grades the children on the results of a half-year's work, with

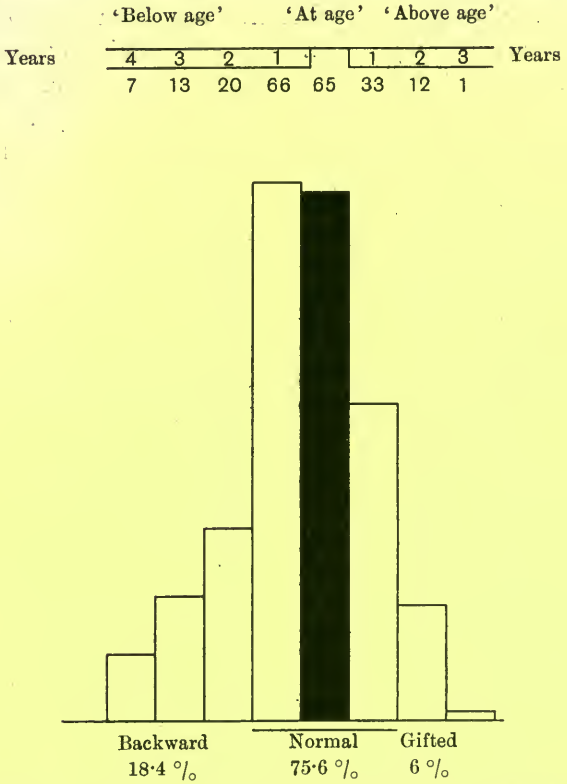


Fig. 4. 1911 Scale. Distribution in Mental and Physical Age.

TABLE IV.

Grade	Class	Number in Class	Correlation Index	Probable Error
1	Junior Infant	26	0·42	0·11
2	Infant 1	25	0·85	0·04
3	Junior 3	28	0·52	0·09
4	Junior 2	35	0·41	0·09
5	Junior 1	22	0·51	0·11
6	Senior 2	26	0·61	0·08
7	Qualifying Class	20	0·34	0·13
8	Supplementary 2	22	0·16	0·14
9	Supplementary 1	5	0·62	0·19

constant observation, the experimenter marks the individual child after from thirty to forty-five minutes' testing. Temporary fatigue, indisposition, shyness, constraint, or indifference may lead a given child to show far below its ordinary capacity; excitement or freedom from school-routine may lead others to do better. The mental causes will affect older children more than younger. (2) The ranking of the pupils was left to the teacher of each class, although the head-master supervised the lists. That a class of pupils might be differently ranked by two qualified persons examining the *same* work is recognised: the same difference of standard would affect the ranking of two different classes by two teachers, in comparison with the ranking of a third teacher, who examined both classes. The chief merit of a satisfactory scale of intelligence tests will be its provision of an objective basis of comparison for such purposes. (3) Much more important than the above causes of discrepancy is a third, viz.: the different values of the tests themselves at different ages. For younger children, the tests, although comparatively easy for each year taken by itself, are far more searching, because they may be extended upwards as far as the pupil's capacity reaches. For older children, the tests at each age are too difficult; they have little connexion, on the whole, with school work; the ability to pass them depends upon other abilities than those by which the teacher is guided; the gradation of difficulty is steeper from year to year, and there are no tests for ages 11, 13 or 14, with the result that a pupil failing in one or two tests at age 10 has very few chances of making good the defect by higher tests. The scale is like a finer sieve for the earlier and a rougher sieve for the later years. Hence, probably, the low correlation-indices of grades 7 and 8. In grade 9 only 5 pupils were tested, and accordingly it may be left out of consideration. The very high index for the infant class 1 may be attributed, partly at least, to the fact that the tests harmonized more closely with the *kindergarten* methods of teaching. In the youngest class these methods had not yet begun to have effect.

3. *Sex Differences.* A comparison of the efficiency shown by the two sexes is of interest in view of the results obtained by Bloch and Preis[1] in their application of the Binet-Simon method, and also because so little in the way of a scientific investigation of sex differences in mental ability has so far been done[2]. Figures 5 and 6 and Table V form

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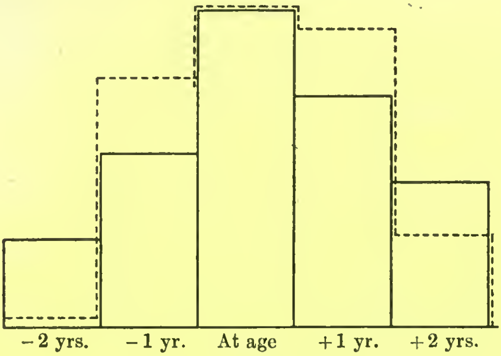
[1] *Ztsch. f. angew. Psychol.* 1912, VI.

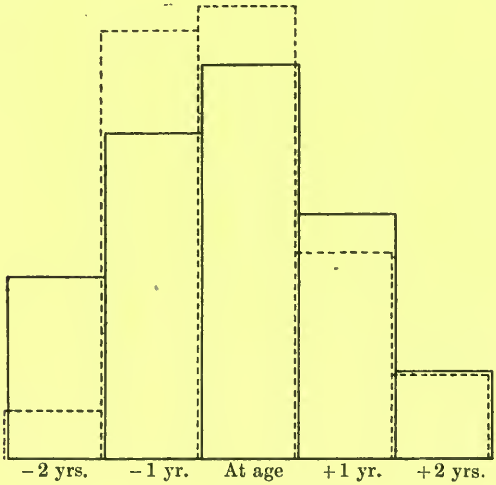
[2] Thorndike, *Educational Psychology*, ch. III.; Thompson, *Mental Traits of Sex*; Burt, *J. of Exp. Ped.* I. 2, are the only systematic surveys.

the basis of our conclusions. The principal outcome is the familiar law that there is a wider scattering of ability among boys than among girls; consequently there are more gifted as well as more backward boys than girls. Goddard's method of representing results makes this sex-difference in variability abundantly evident in the case of the data



	– 2	– 1	At age	+ 1	+ 2
Boys ——	9 °/ₒ	18 °/ₒ	33 °/ₒ	24 °/ₒ	16 °/ₒ
Girls	1 ,,	26 ,,	33 ,,	31 ,,	9 ,,

Fig. 5. 1908 Scale. Ages 4 to 11.



	– 2	– 1	At age	+ 1	+ 2
Boys ——	15 °/ₒ	27 °/ₒ	32 °/ₒ	19 °/ₒ	7 °/ₒ
Girls	4 ,,	35 ,,	37 ,,	17 ,,	7 ,,

Fig. 6. 1911 Scale. Ages 4 to 11.

obtained from our application of the 1908 and 1911 scales (Figs. 5 and 6[1]).

Bloch and Preis's experiments were carried out on 150 elementary school children from 7 to 11 years of age, who had been selected as of average ability and school record. They found almost everywhere remarkable inferiority in the girls.

A table is given by Stern, from Bloch and Preis's results, showing the percentages of boys and girls respectively passing in eleven different tests (*op. cit.* p. 46); we drew up a similar table, but our percentages are so different from theirs that practically no common result emerges.

TABLE V. *Comparison of Abilities in Boys and Girls, of each Age. 1911 Scale. Percentage Frequency of Children.*

Age		Backward	Normal	Gifted
5	Boys	8	84	8
,,	Girls	0	100	0
6	Boys	0	85	15
,,	Girls	0	70	30
7	Boys	0	100	0
,,	Girls	0	90	10
8	Boys	0	100	0
,,	Girls	7	86	7
9	Boys	0	100	0
,,	Girls	0	100	0
10	Boys	10	90	0
,,	Girls	0	85	15
11	Boys	55	33	12
,,	Girls	16	84	0
12	Boys	70	30	0
,,	Girls	45	55	0
13	Boys	60	40	0
,,	Girls	63	37	0

This shows that the numbers of subjects trying each individual question are in both experiments too small for any valid conclusion as to sex differences being drawn. The ideal method would be to set each question to at least 100 girls and 100 boys of each age, of the same social level. Our subjects, of course, were unselected, while theirs were of 'average' ability, but we feel convinced that this fact had but the slightest influence on the result. So far as they go, our experiments support the general law, which several observers have already noted, that there is greater variability of capacity in boys than in girls, but no general superiority.

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[1] Ages 12 and 13 are omitted, owing to the unsuitability of the tests, but the percentages of the results at all ages are shown on Table V, for the 1911 scale.

4. *Comparison of Distribution of Ability in French, German, American and Scottish Children.* In Table VI our grouped results, by the 1908 scale, are compared with those of Binet (Paris), Bobertag (Germany), Goddard (U.S.A.), and Terman and Childs (U.S.A.), by all of whom the 1908 scale was taken as the measuring instrument. 'Normal' children include those 'at age,' and also one year above and one year below age; 'gifted' children are two or more years in advance of their physical age, 'backward' two or more years behind their physical age. It will be seen that our results approximate more closely to those of Goddard, and to Terman and Childs, but show little correspondence with Binet's[1] or Bobertag's.

TABLE VI. *Comparison of Distribution of Abilities in different Nationalities.* 1908 *Scale.*

(−2 = −2 years or more, +2 = +2 years or more.)

	−2	−1	0	+1	+2	Number
French: Binet*	6 %	23 %	48 %	22 %	1 %	192
	'backward'	93 % 'normal'			'gifted'	
German: Bobertag†	2½ %	18 %	52 %	25 %	2½ %	228
	'backward'	95 % 'normal'			'gifted'	
American: Goddard‡	19 %	20 %	36 %	21 %	4 %	1547
	'backward'	77 % 'normal'			'gifted'	
American: Terman and Childs§	10 %	16 %	33 %	28 %	13 %	396
	'backward'	77 % 'normal'			'gifted'	
Scottish: M. and R.	18 %	21 %	28 %	22 %	11 %	217
	'backward'	71 % 'normal'			'gifted'	

* *L'Année Psychol.* 1908, 73. There is, however, an error in the numbers stated: the *reguliers* ('at age') add to 93 instead of 103 as given; and there is another slight error. We have assumed the detailed figures to be correct.

† *Ztsch. f. angew. Psychol.* VI. p. 501.

‡ *Ped. Sem.* 1911 (the higher grades are omitted as in Meumann, *op. cit.*).

§ *J. of Educ. Psychol.* 3 (2 to 5) 1912 (from the data on p. 69).

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[1] Binet's subjects were 'selected.'

It has been pointed out that this massing of results is misleading, as the earlier sections, with their easier questions, give a relatively larger number at or above age, while the later sections, with more difficult tests, give a relatively larger number below age; when these two unlike samples are put together, we have an apparently symmetrical grading of distribution which is quite illusory. Even so, it is plain that the tests are better adapted to the sifting out of 'backward' or 'gifted' children in America or Scotland than in France or Germany. If we exclude from our table all children over the age of 10, for whom, as has been shown, the tests are not well adapted, we have the following more reliable results for 140 children:

− 2 years	− 1 year	'at age'	+ 1 year	+ 2 years	+ 3 years
½ %	11½ %	39 %	33½ %	14 %	1½ %
½ %		84 %		15½ %	
'backward'		'normal'		'gifted'	

This corresponds much more closely than the results for all the ages do with the Binet and Bobertag normal values. It also shows that the tests were, on the whole, easier for our younger children than for the corresponding ages in France, Germany and America, in spite of the fact that all were drawn from approximately the same social level. Stern[1] has eliminated from Goddard's figures those belonging to ages 12 and upwards, and obtains:

No.	− 2 years	− 1 year	'at age'	+ 1 year	+ 2 years
1277	11 %	20½ %	41½ %	21½ %	5½ %
			83½ %		
	'backward'		'normal'		'gifted'

If we include age 11 in our results, we have:

177	4½ %	22½ %	33 %	28 %	12 %
			83½ %		
	'backward'		'normal'		'gifted'

We have still an exceptionally large percentage of 'gifted' children, in spite of the fact that age 11 has not added any to the total. It is true, of course, that this may be accounted for by mere chances of sampling, and not necessarily by any national differences in the quality of the intelligence.

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[1] *Op. cit.*

IV. THE QUESTION OF STANDARD.

It will be understood that the Binet-Simon scale affords only an *indirect* means of measuring intelligence; at the same time it claims to be a more objective method than any hitherto adopted, standardising both the measure to be applied and the mode of procedure in its application.

The assumption is that at each age there is a given degree of mental maturity, just as, for example, there is a 'normal' temperature at a given place for each month of the year. As a given month may show in exceptional circumstances a temperature higher or lower than the standard, so a given child may at a given age have passed beyond or have fallen short of the standard degree of maturity. The value of the scale as a measuring instrument depends entirely on our ability to determine this standard for each age, and it is obvious that, in the long run, the standard can only be determined by a very large number of observations. Needless to say, also, the observations should be made with the same measure applied in the same way in the different cases. Until this large number of observations has been made, our standard cannot be more than hypothetical; but we have already a certain amount of more or less unscientific experience by which to be guided. If we assume for the moment that the curve of distribution of intelligence is a 'normal curve,' and if, applying our measure, we find that as a matter of fact our distribution approximates to such a curve, then our standard is so far established.

If we take the whole number of our results as a basis, as shown in Figures 3 and 4, and estimate from the standard deviation the theoretical distribution over the different mental ages, we have the following table, the mean for the first scale being 0·2 below age, and for the second 0·5:

1908 *scale.*

					'at age'			
Years	−4	−3	−2	−1	0	+1	+2	+3 years
Calculated	1·5	8·2	26·5	52·2	61·2	42·7	17·5	4·4
Observed	1	12	27	45	59	49	20	2

1911 *scale.*

					'at age'			
Years	−4	−3	−2	−1	0	+1	+2	+3 years
Calculated	2·3	11·6	34·2	58·9	59·5	25·3	12·3	2·5
Observed	7	13	20	66	65	33	12	1

It will be seen that the 'observed' correspond fairly well with the 'calculated' frequencies, and that of the two scales, the 1908 figures are closer than the 1911. At least, there is every indication that with a larger number of observations we should closely approach a normal curve. Further, if with Goddard we regard as of 'normal' intelligence the three groups, – 1 year, 'at age,' and + 1 year, then the calculated percentage of the whole is for the 1908 scale 72·7, observed 71; for the 1911 scale, calculated 70·8, observed 75·6. Our data are therefore quite consistent with the assumption which Goddard and others make, that the central group of normal intelligence should contain 70 or 75 °/。 of the whole, and, on this assumption, the Binet-Simon scale is satisfactory.

As to the shape of the ideal curve, to which our data offer an approximation, the application of the Pearson formulae[1] shows a skew-curve of type I for both scales. The skewness is however very slight; in the earlier scale the mean is – 0·2 years and the mode + 0·05; while for the later scale the mean is – 0·5 and the mode – 0·2. For practical purposes, therefore, we may assume that with numbers sufficiently large we should find a normal curve (type VII).

A discussion, on general grounds, of the whole subject of the distribution to be expected on the application of such tests, will be found in Bobertag (*op. cit.*). The conclusions, however, are rather indefinite.

Two difficulties still remain: (1) We do not, as a matter of fact, apply the same measure to the children of different ages, hence the material is not homogeneous. There are different tests from year to year, a pass means a very different thing at one point or another of the scale. This difficulty could be satisfactorily overcome by subjecting every child to exactly the same tests. We suggest below a means by which this may be accomplished without sacrificing the variety which we have seen to be a requisite in testing so complex a faculty as intelligence, and at the same time without fatiguing the child unduly by the tests. In our scheme there are fifteen different tests, five for each of three successive years, and repeated with a higher standard at three-year intervals. The number does not appear too large for every child to have opportunity of trying all fifteen tests. The results would allow of an accurate grading of the subjects, which the present scales fail to admit. The standard for a pass at a given age should be determined on the basis of actual experience; our own results agree with those of Bobertag and Goddard, as about 70 or 75 °/。. Five passes, of

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[1] See W. P. Elderton, *Frequency Curves and Correlation*, London, 1906, Ch. 4.

different tests, would then count as one mental year. (2) The second difficulty is that the unit of measurement is not the same at different points of the scale. Thus, to take a single case: That a child of five years should have the intelligence of three years is much more serious than that a child of twelve should have the intelligence of ten years only; yet in both cases the measure is '– 2 years.' The difference of a year means less as the child grows older. On this ground Stern (*op. cit.*) suggests that we should take as our measure not the *difference* between mental and physical age but the *quotient*, mental divided by physical age. 'At age' would be represented by 1·00, 'below age' by a fraction less than 1, and 'above age' by a sum above 1. Our example would give for the child of five 0·6, and for the child of twelve 0·83. This would be an improvement, if it were generally adopted, although it would convey a much less obvious meaning to the parent or teacher.

Whatever method we adopt, the first essential is that the tests be properly chosen, the second that the method of application be properly standardised, and the third that the criterion of success in a given test be accurately determined. It is, however, only the experience of actual applications that will help us towards these essentials. We have indicated above three comparisons from which information of the value of the tests may be obtained, correlation of the scale results with the teacher's classification, the grouping or distribution of intelligence at each age, as brought out by the tests, and comparison with the results of other observers—probably the most important of all, provided that the differences arising from social level and opportunities, in the pupils, are kept in mind.

V. SUITABILITY OF THE TESTS.

1. *Tests in which Aberdeen children show Backwardness or Precocity.* In Table VII, the position of a number of tests in the 1908 scale is indicated, and their age-rank according to the results of our application is given for comparison. A test was considered properly assigned to a given age, when at least 70 °/₀ of the children of that age were able to pass it[1]. In the case of those tests which are not in the table, this condition was satisfied.

This table indicates the imperfections of the scale for our requirements from a new point of view, tracing them to their source in the individual tests. The weakness of the scale in the earlier years is shown to be due to the faulty grading of tests 15, 16, and from 21 to 27. Seven of the eight tests for age 7 are inappropriate; they can be answered by Aberdeen children a year earlier. That the scale is too difficult for the later years is likewise explained by the defects in grading tests 45—56. Only one of the tests assigned to age 11 is suitable for our subjects of age 11, and all the tests for ages 12 and 13 can only be performed at higher ages.

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[1] Other investigators have adopted 70 or 75 °/₀ as standard, *e.g.* Goddard, Bobertag. In comparing the results of different applications, the standard used is most important.

TABLE VII.

Age	Order of Question		Changes in Grading
IV	9	Comparison of lines	← III
V	13	Counting of pence	← IV
VI	15	Repeating 16 syllables	VII →
	16	Aesthetic judgment	← IV
VII	21	Omissions in pictures	← VI
	22	Counting fingers	← VI
	23	Copy writing	← VI
	24	Copy diamond	← VI
	25	Repeating 5 digits	← VI
	26	Describing picture	← VI
	27	Counting pence, 13	← VI
VIII	29	Reading, 2 memories	IX →
	30	Counting money	IX →
	31	Naming colours	← VI
	32	From 20 to 1	IX →
	33	Dictation	← VII
IX	36	Days of week	← VI
	38	Definition better than use	XII →
	40	Arrange weights	XII →
XI	45	Trick phrases	XII →
	46	Sentence-building, 3 words	← X
	47	60 words in 3 minutes	XV →
	48	Define abstracts	Adult →
	49	Arrange words	Adult →
XII	50	Repeat 7 digits	XIII →
	51	Make rhymes	XIV →
	52	Repeat 26 syllables	Adult →
	53	Supply missing words	XIV →
XIII	54	Folding-paper puzzle	Adult →
	55	Transposed triangle	Adult →
	56	Distinguish abstracts	Adult →

The tests in which our subjects are in advance of or fall behind children of other nationalities of the same age are noteworthy[1]. Precocity is shown in those tests that depend chiefly on habit, training, or memory, while backwardness is conspicuous in those appealing to 'natural intelligence,' alertness, and adaptability. Thus, copying a written phrase, counting fingers or pence, saying the days of the week, naming colours, and writing to dictation, which comprise 8 of the 14 tests that require to be put back, depend for success upon memory or training. On the other hand, reciting the numbers from 20 to 1, defining words simple and abstract, arranging weights, and the like, demand a certain amount of initiative and inventive power, and in these our subjects were decidedly backward.

Two things might account for this unexpected result. The more important is the influence of the school atmosphere. Not only were the 'school'-tests familiar and likely to be more promptly attacked and more confidently performed; the mere fact of being in school seemed favourable to a receptive attitude and unfavourable to independent effort and initiative. Apparently, where group action is the order of the day, it is difficult to break the chain of custom and exercise individual judgment and will. Further, the experimenters were strangers to the children, and the fear of 'giving themselves away' certainly accounts to some extent for the lack of spontaneity in answering such questions as 38, 47 and 52, all of which require assurance and self-confidence. These two factors, however, must also have affected the results of other observers, although possibly to a less extent. As a third influence, indirectly of importance in this connexion, we would suggest the national temperament. It may be an instance of the well-known tendency of the Scot towards reserve and 'canniness,' that such tests as 47 (saying as many words as possible in 3 minutes) were so inadequately performed. The reserve and restraint thus caused were intensified also by the general backwardness in language. In all the tests in which speech is an important element, retardation is invariably shown by the Aberdeen children. The verbal repetition of a sentence of 16 syllables, a test assigned by Binet and Simon to age 6, is only passed by 2 out of 23 of our subjects at that age. This failure must be attributed partly to the fact that these children are bilingual. The tongue with which they are familiar is the Scots tongue. Sixteen syllables in 'braid Scots' would not have presented the same difficulty to them. Unfortunately this language-difficulty enters into 43 of the 56 tests which comprise the scale. We accordingly agree to some extent with Ayres's[1] criticism that the importance it assigns to language is exaggerated. Binet himself admits (*Année*, 1911, p. 129) language to be so influential that changed results are to be expected when the scale is applied in a new social environment. Such tests are:

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[1] Cf. Bobertag, *Ztsch. f. angew. Psychol.* 1912, 497, 505; J. Carleton Bell, *J. of Educ. Psychol.* Feb. 1912, 104 and 105.

(1) Understanding of words representing simpler and more complex objects and ideas. (14, 17, 20, 38, 43, 45, 46, 48, 49.)

(2) Finding names for objects, qualities, and differences. (7, 28, 31, 34, 42.)

(3) Description and interpretation of pictures. (26.)

(4) Report of an experience and of conduct in imaginary situations. (29, 39, 44, 53.)

(5) Repeating as many words as possible in 3 minutes. (47.)

These are all valuable indications of intellectual equipment, and assuredly the ability to describe and interpret experience is necessary for mental efficiency and success in ordinary life. Indeed, no one would deny that, as the instrument of intelligence, language ought to play a considerable part in any measuring scale. The influence it exercises, however, is too great. It outweighs in importance tests of other capacities to such an extent that the child with a ready tongue has a distinct advantage over the child who is quick of hand though slow of speech. It may be true, as Miss Schmitt[2] contends, that "the success of the child in doing the work of the school above the first 3 or 4 grades depends almost wholly upon this language ability," and that therefore "tests which can adequately disclose or measure this ability should be of great value in a psychological clinic"; nevertheless the aim of the measuring scale is the diagnosis of intelligence or the determination of capacity in the individual child, and for this purpose a more adequate measure of practical intelligence, of address in action, of muscular dexterity, is required. Copying a written phrase, writing to dictation and reproducing a design—the sole tests involving muscular control in the scale[3]—are quite inadequate to measure this important form of intellectual power. We regard this dearth of tests of motor dexterity as a striking defect.

Bobertag[4] contends that recent analysis of intelligence justifies the exclusion of tests of deftness of hand, since motor dexterity, like sensory acuity, has been shown to have a low correlation with intelligence[1]. In fact, however, as Abelson[2] has pointed out, the principle that ought to be observed in this connexion is the avoidance of tests that can be performed in a mechanical way. In the case of children, the learning of a new movement involves some form of intelligence, and accordingly tests of skill in constructive ability and planning should form part of the scale, while several of the tests at present in use, such as reciting the months or the years, should be rejected or modified, as they both depend on mechanical memory, and also imply an experience which the child may not, in fact, have had.

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[1] Ayres, *Psychol. Clin.* 1911, No. 6.

[2] Clara Schmitt, *Ped. Sem.* June, 1912, 188.

[3] Stern, *Die psychologischen Methoden der Intelligenzprüfungen.* Stern refers to a test of muscular ability which apparently does not exist.

[4] Bobertag, *L'Année Psychol.* 1912, 275.

The backwardness of our subjects in command of language is nowhere more evident than in the definition test. It deserves special comment, because it reveals the urgent need for analysing the tests and standardising the procedure used in their application. Its purpose is to ascertain the child's ability to express simple ideas in language. It does so by asking him to explain the meaning of certain words representing concrete things. Since the ability to comprehend and use language increases with intelligence, this test seems to promise a means of measuring degrees of mental efficiency.

In our application of the test, the procedure adopted by Binet and Simon was rigorously observed[3]. Thus, after it had been made certain that the child knew the word to be defined, he was asked to tell its meaning. For example, the experimenter would inquire, "You know what a chair is, don't you?" and if this was answered affirmatively he then would ask, "Well, what is it?" If the child remained silent, an illustration was occasionally given in the earlier years.

In valuing the various responses, a number of difficulties arise. According to the French authors of the scale, three kinds of reactions are to be expected. In the first case, the child remains silent or repeats the given word, where, for instance, he gives the answer "a chair" in response to the question "What is a chair?" or, again, points to an actual chair. In the second type of reaction a definition in terms of function is returned; *e.g.* "A fork is for eating with," or "A horse is for riding." In the third class of responses, the child's explanations always contain *more* than a statement of the function of the thing in question.

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[1] Burt, This *Journal*, 1909, 135.

[2] Abelson, This *Journal*, 1911.

[3] Miss Isabel Lawrence applied this test to 784 children: but unfortunately her method of application was not Binet's: *Psychol. Clin.* Dec. 15, 1911.

There is a short description of its appearance or an enumeration of its parts or a statement of the material of which it is made or of the class or genus to which it belongs. These classes of reactions correspond to definite ages. Thus at age 6 the second type of response is given by a large majority of normal children, and at age 9 the third kind of reaction is most common. Binet and Simon accordingly adopted the plan that at age 6 the definitions given must be at least functional, and at age 9 they must be by 'something better than use.' But this apparently simple criterion admittedly involves considerable ambiguity[1], and in many cases where ambiguity is not present the method appears to be unfair. There is certainly injustice in 'passing' a very poor descriptive definition while 'failing' a very good functional one at age 9. To say "a fork has things like fingers," indicates no higher mental ability than to say it is "for eating with," yet Binet's criterion, strictly applied, estimates the former as adequate and the latter as inadequate for the mental age 9.

In view of these facts, an analysis of our responses was undertaken in order to ascertain how far this criterion of 'use' and 'better than use' is applicable. It was found that six different types of answers could be distinguished. Consideration of these casts doubt upon the Binet-Simon procedure in applying the test, and calls in question the reliability of the criterion they have adopted.

1. Of all the kinds of definitions, the first to appear is the purely functional definition. Its peculiarity is that it indicates an attitude which prevails only in the earliest years. That is a subjective attitude towards all external objects. Such definitions are "A chair is for sitting on," "A horse is for riding on." Children at ages 4 and 5 are incapable of giving a definition except in this form, because the meaning of objects is as yet determined by the relation of things to their activities. The use to which they put a thing constitutes for them its essence, its total nature or being. This is the prevailing attitude for all children under 6. Thereafter, the number of children who define in terms of use steadily decreases, although such definitions continue to be given at all ages.

2. The second type of definition involves, in addition to a statement of function, a distinct recognition of the objective nature of external things. Objects are regarded as existing in and for themselves. Their 'meaning,' however, still remains subjective in character. Such definitions are "A school is a place for children to come to," "A table is

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[1] *L'Année Psychol.* 1908, 25.

where you put dishes on when you go to take your dinner." A primitive classification underlies such explanations. A rough separation of external objects into things, animals and places has been made. It is entirely distinct from a logical classification, if only because the child is not even aiming at a classification. Any classification there may be is purely accidental; yet it is important as indicating a changed attitude to the world without.

3. The third type is that of pure description. No trace of classification characterises the purely descriptive definition. It states either the parts, the form, the material, or the appearance of the object in question, *e.g.* "A horse has long hair and it has a black tail," "A chair is just a board with a back and four legs." It is now a purely objective attitude that is adopted towards things.

Within this class, definitions may vary considerably in value. Thus the definition "A fork is a handle, three bits at the top of it, iron," whatever its faults, is superior to "A table has four legs" or "A chair has four legs too." Both are purely descriptive definitions. This manner of stating the significance of words is characteristic of ages 7 and 8; it is never found before 6 and never entirely disappears at higher ages.

4. The fourth type of definition is also descriptive, but there is a marked advance in complexity. Description is conjoined, as it were, with a primitive or pre-logical classification of the kind already found in the second type of definition. For example, a school is defined as "A big place with classes in it," a fork is "A thing with sharp edges," and a horse as "A beast with four legs." In purely descriptive definitions a salient feature is stated, and the impression one receives is that of a diffuse recollection rather than orderly description. In mixed descriptive definitions objects have been roughly divided into the classes, things, places, animals, etc., and to such a rough classification a descriptive element is added, marking off the object more or less adequately from other objects belonging to the group. It is characteristic of age 11, and it continues to appear at higher ages.

5. In the fifth type, classification is definitely sought after as a means of expressing the significance of words. Thus a chair is defined as "a bit of wood," a school as "a building," a horse as "a beast," and a fork as "a bit of silver." These definitions are rare, but they clearly belong to a group apart. In their case a classification is achieved, but no specification is added. There can be no doubt as regards the child's search for a higher class; the nature of the various responses by the

same child shows that frequently the *genus* can be given without a *differentia.* This type of definition implies an essentially objective and selective attitude. It is very rare at any age, because when a child can give the class to which a thing belongs, he is usually able to assign a specification also.

6. The sixth type is definition proper, *e.g.* a fork is "an instrument which has a handle with three prongs or four for taking your meat with, along with a knife." Only at age 11 was this kind of response relatively frequent in the case of Aberdeen children. It certainly prevails more and more as age increases.

TABLE VIII. *Definition Test.*

Age	Type of Definition					
	1 Pure Functional	2 Prelogical Classification and Function	3 Pure Description	4 Prelogical Classification and Description	5 Logical Classification, no Specification	6 Logical Definition
	°/o	°/o	°/o	°/o	°/o	°/o
4	94	6	—	—	—	—
5	91	—	—	—	8·5	—
6	80	13	6·5	—	—	—
7	36	15	49	—	—	—
8	38	46·5	15	—	—	—
9	48	45	3	3·5	0·1	—
10	45·5	30	6	9	3	0·6
11	28·5	24	17	17	1	14
12	39	25	21	10	—	3·5
13	5	42·5	2·5	30	2·5	17·5

Table VIII shows the frequency of the various types of definition. It suggests that the general trend of language development is, as Binet and Simon describe, a progress from functional definition through descriptive definition to classificatory definition. Functional explanations decrease in number with age, whereas descriptive and classificatory definitions increase, and again descriptive definitions appear earlier than classificatory. This may easily be shown by combining in Table VIII the percentages of 1 and 2, 3 and 4, 5 and 6 respectively.

From a statistical point of view, however, the criterion adopted by Binet and Simon requires amendment. While it is satisfactory at age 6 it is much too difficult for age 9: in the case of our subjects, definition 'better than use' is only to be expected by age 11. It is clear that more serious drawbacks attach to this criterion of 'better than use.' The chief of these is its ambiguity. It depends entirely upon the nature of the description or of the function given. In this connexion Bobertag makes some useful criticism, but he seems to overlook an important fact in his analysis. He divides all definitions into two types, descriptive and classificatory, and includes functional definitions under the former. This neglects the significant change from a subjective to an objective attitude, as regards external things, which had impressed Binet and Simon[1].

Our results confirm their view that there is an age, namely from 4 to 6, when any definition but a functional one is beyond the child's power, when his attitude to external objects is essentially subjective, when the nature of things for his mind consists in personal experiences, and can only be stated in terms of such. Later, this attitude changes. In normal children it ceases to be completely subjective, but the extent of the change varies greatly from individual to individual. These variations are so great as to suggest different types of mental character. Binet called attention to such differences in his *Étude expérimentale de l'intelligence.* Of his two subjects, M. and A., the former always adopted an objective attitude, observing and describing in minute detail. She regarded things in a frankly impersonal way. A., on the contrary, adopted a subjective attitude, giving a very meagre description of details, but expressing the feelings evoked by objects, their subjective value or meaning. Such dissimilarity of outlook explains the variety of responses received in our definition-test from normal children of the same age. Clearly then, if we had classified our results by Bobertag's method, the bearing of the test on the problem of determining intelligence by age-grades would have been entirely lost. There is no advance from descriptive to classificatory definition in his sense of the terms.

We conclude that Binet and Simon were wrong in regarding description as necessarily 'better than use.' From the presence of description we infer a certain mental level; but from its absence we can deduce nothing, as functional definitions may occur at any level of intelligence. Consequently at ages higher than 6 it would seem expedient to judge by the quality of the definition rather than by its form. Yet even if the criterion of success is improved, the definition test cannot be standardised as long as the existing method of application is followed. It is possible, however, so to improve the method of applying the test as to make the classificatory definition a real mark of the attainment of a definite degree of ability. Bobertag makes valuable suggestions in this connexion; Binet and Simon's choice of words obviously admits of improvement; they did not perceive that some words naturally and more readily admit of one type of definition rather than another. This is a vital fact which makes it absolutely necessary that the words to be defined should be carefully chosen so as to test the child's power to give the various types. The number has, of course, to be limited if the value of the scale is to be retained. Further, a determinate method of putting the question must be strictly followed.

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[1] *L'Année Psychol.* 1908, 81.

2. *Tests depending on School Training.* Besides this attack upon the exaggerated importance assigned to language, which our results appear to justify, various other objections have been brought against the measuring scale. The most important objection made by the critics of the 1908 scale is that it depends too much upon knowledge obtained in school and upon the child's familiarity with a social environment of a certain level. Thus Decroly and Degand[1] criticised it adversely because certain tests appeared to depend too much either upon the length of time a child had been at school or upon the kind of training he had received there. It was urged that tests such as 'repeating a spoken sentence' or 'copying a written phrase' had little relation to 'natural' intelligence: an illiterate child might be highly intelligent and yet fail in these; and since the scale should be directed towards investigating the 'function,' intelligence, and not the 'product,' culture, such tests were condemned as unsuitable. Nevertheless, in a country such as our's, in which education in the three R's is universal, this objection loses much of its force. We found these tests to be among the most thorough and most easily applied. The child is in his most normal attitude when confronted with such tasks in school, and because of his familiarity with the type of question they seem a remarkably fair measure of capacity. They are less likely to be influenced by temporary fluctuations, and, as preliminary tests, they are valuable aids towards inducing a work-attitude, especially as many of the other tests resemble parlour puzzles, and tend to produce the attitude characteristic of play. Every test implies previous experience of some sort, which the child may or may not have had. In the school-tests, we are at least certain that the experience has been offered to him, whether he has profited by the opportunity or not; and there can be no doubt whatever that the ability to read and write after a certain amount of

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[1] *Arch. de Psychol.* 1910, IX. No. 34.

instruction does involve some part of that complex capacity we call 'intelligence.' We should therefore prefer on general grounds to retain the majority of these tests, notwithstanding the fact that the results of our application of the two scales is not decisive on this point. There is difficulty in analysing the complexity of causes at work and in estimating and interpreting their effects, but at least there seems justification for asserting that the extension of school-tests is supported rather than condemned by the results we obtained. Such superiority as is observed in the revised scale of 1911 need not be ascribed to their omission, since it might well be due to the extensive improvements in the grading of the remaining tests. Thirteen of the twenty-three changes in grading, made by Binet and Simon in 1911, we likewise found necessary in the case of our subjects (Table VII, Nos. 24, 27, 31, 40, 45, 47, 48, 49, 51, 52, 54, 55, 56). Not only so, but any improvement found in the 1911 over the 1908 scale might be due not to the omission of 'school-tests' as such, but to the removal of school-tests which were badly graded in 1908. Four of the school-tests we found too easy for Aberdeen children at the age to which they were assigned (Table VII, Nos. 13, 22, 23, 33). The slight improvement of the later over the earlier scale might, therefore, be traced to some extent to the elimination of these particular sources of error.

Having regard to these facts, we consider Binet and Simon's original idea and ultimate decision, to have a separate measure of ability in school subjects, and to place tests involving school instruction only in that scale, and not in a 'measuring scale of intelligence,' as mistaken, and we agree with Miss Schmitt that the "Intelligence scale would in itself be an inadequate measure of intellectual ability, if, while professing to gauge native capacity, the tests were concerned very little with the things that all children are expected to acquire, and which every experience has shown they do acquire"—the abilities to read, write, and count. If two scales are constructed, both must be applied, and their combined information utilised, before an accurate knowledge of the child's mental efficiency can be had.

Indirectly, we attributed to the school-tests in part the failure of the 1908 scale to give an entirely satisfactory distribution of ability (p. 285 and Fig. 1). The weakness of that scale in the earlier years was shown to be due to its being much too easy, and it is significant that tests 13 (age 5), 22, 23, 24, 27 (age 7), and 33 (age 8), all tests depending on school training, were in every case found suitable for children a year younger. The important point in this connexion is that this result is not surprising but natural, and to be expected. Any scale which includes tests depending on training will require changes in grading when it is applied in a new environment. Schooling begins at different ages in different countries, and consequently tests of ability in school subjects could never have a universally applicable standard. Their value is not thereby destroyed. Indeed, here we have an instance of a more general principle recognised by Binet and Simon, that the social environment is a factor which must be reckoned with in appraising results in the case of any scale, however perfect. They were aware that their scale would not give identical results in different environments, and that there may be conditions in special environments of such importance as to influence results considerably[1].

A good example of this is to be found in the application of their method to Roman children by Alda Jeronutti[2]. Comparing the distribution of abilities in Italian children, which she obtained, with those given by Binet in the case of French boys and girls[3], we have the following percentages[4]:

	Below Age	At age	Above Age
Binet	28	50	22[5]
Jeronutti	17	29	54

The difference of these results shows in a striking way that the scale requires adjustment to suit Italian conditions. It is not to be expected that Roman and Parisian children of the same age will be equally successful in tests which were originally devised to suit conditions in Paris. With every conceivable scale, differences in nationality and social level and differences in education must have *some* influence, and must necessarily lead to different results, just as in physical measurements nationality, mode of life, etc., are found to be important factors. The average height and weight of Russian males as compared with Japanese, or of town children as compared with country children, differ, but this in no way detracts from the value of the physical standard. Neither should it be held to impair the value of the measuring scale of intelligence that different results are obtained in different communities, since the 'averages' or 'normals' of different communities must differ in certain respects.

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[1] *L'Année Psychol.* 1911.

[2] *Riv. di Ped.* 3 Dec. 1909.

[3] *L'Année Psychol.* 1908.

[4] *Vide* Meumann, *op. cit.* p. 154.

[5] Binet's total figures do not correspond with the sums of the individual figures in his table (*L'Année Psychol.* 1908, p. 73). If the totals are correct, the above holds: if the individual figures are correct, the percentages are:—backward 29, 'at age' 48, advanced 23.

3. *Tests depending on Home Surroundings.* Although they recognised the impossibility of entirely eliminating the influence of environment, Binet and Simon considered it desirable to discard as far as possible tests contaminated by obvious traces of training, on the grounds that they gave no reliable indication of the presence or absence of intelligence, and that opportunity or accident was too prominent an element in such tests. Consequently, from the 1911 scale several tests were excluded, on the ground that success seemed to depend almost entirely on the character of the child's *home* surroundings. Decroly and Degand had found that a larger percentage of children of a good social class are 'passed' by the tests than of children of a lower level. Such tests are No. 6, where the child is asked "Are you a boy or a girl?" and Nos. 19, 22, 35, and in which he is required to tell his age, the number of his fingers, and the date. It is true that ignorance of his age or the date in the case of a child of 9 is not necessarily an intellectual defect, for he may merely have forgotten the last relevant information he had on the subject; but, on the other hand, it is a fairly good sign of some kind of intelligence if a child of 9 *does* know the date and can give his age accurately. Although no two individuals have environments entirely alike, still there is a certain amount of social experience common to all who have a sufficiently normal intellect to grasp it. Tests can surely be found making a wide enough appeal for the children of one race and civilisation. Failing this, the best solution of the difficulty would be to have alternative questions, referring to matters of social or commercial importance, in which a child could be counted as 'passed' if successful in any one of them. The need for such alternatives was brought home to us in applying test 37, in which, having been provided with a number of coins in common use, the subject is asked to give the correct change on a purchase made (2*s.* is presented for an article costing 4*d.*). Many of the parents were small shop-keepers, and their children could usually give the required sum accurately and rapidly, whereas other children, though more advanced in the school-tests, were comparatively at a loss in this.

If the home environment tests are retained, we see little ground for Ayres's criticism[1] that the scale largely consists of puzzle tests and tests of immediate memory, which have little relation to intelligence, and "overlook the fundamental difference between the multiple and complex stimuli, which contribute to the motivating impulse in coping with real problems, and the few simple ones entering as factors in answering questions and obeying commands." They are neither puzzle tests nor tests of immediate memory, and even complex motives come within their scope, notably in the case of the problem questions (44). Many of the replies reveal in a surprising way the moral level of the child as well as his practical wisdom. Such questions as "Why should you judge a person by what he does rather than by what he says?", or again "If a boy hits you without meaning to, what should you do?", receive very varied responses, and disclose to an unexpected extent the child's natural disposition and the motives that appeal to it. Success in these tests is not dependent upon recent environmental experience. The tests which turn on the reckoning of time or the use of money may in part depend upon recent experience, but the problem questions have not this drawback, and all of them demand intelligent judgment. They can indicate in no uncertain manner the child's grasp of facts of social value, revealing his normality or abnormality in an important direction.

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[1] *Psychol. Clin.* Nov. 1911, 193.

But Ayres's objection was itself forestalled by Binet and Simon. They admitted that the measuring scale cannot gauge those more complex and subtle mental qualities upon which success both in school and in after life so largely depend. "Les insuffisances d'attention, de caractère, de volonté ne se voient pas ou se devinent à peine dans notre examen d'intelligence. L'épreuve est trop courte, et l'enfant n'est pas assez abandonné à lui-même[1]." The scale merely aims at being an aid to diagnosing degrees of ability, and moreover, in general, we need no information about the child's attitude or response to the innumerable subtle influences of daily life. These are sufficiently known, as a rule, before the tests are applied. We merely turn to the tests for confirmation of the views we hold as regards the child's normality, gained from observation of his social reactions.

The omission of tests referring to the affective side of mind is somewhat unfortunate. The sole instances are the tests of aesthetic judgment (16), and the tests of suggestibility in the 1905 and 1911 scales; the latter, indicating the power to judge and the degree to which feeling and will may be influenced by an authoritative person, refer to a side of mental life which is of vast importance in determining efficiency and social usefulness. More tests measuring the affective and conative capacities would be of value, if only such were available. Several have been suggested by subsequent investigators. Among these the most promising is the 'interpretation of fables' test recommended by Terman and Childs, who also give an interesting measure of practical judgment in the 'ball and field' test[1].

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[1] *L'Année Psychol.* 1908, 75.

The limitations of any scale must be recognised, if only because it is so important practically. It has actually been suggested that the Binet-Simon method can give as adequate a means of deciding between scholarship candidates as a written examination lasting many hours. This ignores the profound importance of those more permanent elements in mental efficiency which no measuring scale can touch, and which, notwithstanding, are the determining qualities for future success and effective work, such as tenacity of purpose and the power of sustained attention.

VI. CONCLUSIONS AS TO THE CHOICE OF TEST AND FORM OF SCALE.

One feature that has impressed subsequent investigations very forcibly is the extent to which chance seems to have decided both the form and the content of the original scale; in this, however, Binet's critics are wise after the event, since they have had the advantage of seeing the scale in actual operation. The chief point upon which discussion has turned is the necessity for repeated tests. There seems universal agreement that a collection of tests, however carefully graded as the result of experience, cannot furnish an effective instrument for determining the degrees of capacity, unless they are selected on some basis whereby the progress of the same ability can be followed through different ages. For this purpose, tests must be repeated. The absence of system that marks the scale in its present form makes it impossible to compare results at various ages, because the tests applied change from age to age and refer to entirely different functions. As long as this is adhered to, a means of measuring finer degrees of mental efficiency is out of the question. Even the distinction of normal from abnormal children is regarded by at least one observer as necessitating the repetition of tests. The investigation of children in Mentally Defective Schools in London led Abelson[2] to conclude that tests graduated according to age are not so valuable as the employment of the same tests at different ages, allowance being made for age. Few, but carefully graded, tests were found by him to be most effective for discovering degrees of defect in children on the border line between normality and abnormality. It is remarkable, therefore, that Binet should have endeavoured to exclude repeated tests from the scale as a kind of blemish.

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[1] *J. of Educ. Psychol.* 1912, 133, 202.

[2] This *Journal*, 1911, 311.

Three forms of scale thus seem possible: (1) the Binet-Simon type, based on the principle that no test is to be repeated; (2) the Abelson type, in which the same tests are applied at all ages, allowance being made for age in the degree of perfection demanded; (3) the type in which a limited number of tests is chosen, and these are applied at different levels. The last we regard as the most satisfactory method of measuring degrees of mental efficiency. We would suggest that 15 kinds of tests in all should be selected, 5 for each year, and that these should be applied at the levels 4–6, 7–9, 10–12, 13–15, 16–18. Such an arrangement enables us to trace the growth of an ability with increase of age, and at the same time does not endanger the usefulness of the scale as a quick means of diagnosis. It is beside the mark[1] to object that such a scale cannot detect the causes of defect; it is premature to demand that "a scale should be secured so that the gradation and classification of pupils, the methods of teaching, and the nature of the curriculum, should be based on an accurate knowledge of the mental capacity of the pupils." Such an 'ideal' is too remote. Meantime, effort must be directed towards obtaining an efficient method for distinguishing normal from defective children and average from gifted. The practical importance of the task makes this almost urgent.

The next step, it is clear, must be that the choice of tests, standard, and procedure in their application be conjointly decided upon by a number of psychologists. Many of the Binet-Simon tests would be retained, since, except in the case of the most mechanical, they admit of being graded[2]. Tests of motor capacity and practical constructive ability would have to be added, and some for the detection of moral defectives would likewise require to be found. Improvements in detail could be quickly made if several observers in different parts of the country limited themselves to the study of the tests of a single year or even to one or two tests very widely applied. Miss Lawrence[3] made such an intensive study of the definition tests, and this appears to us a most fruitful line of inquiry. The determination of the performance of the average child at the different ages in the case of each test is essential to further advance.

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[1] Pyle, *J. of Educ. Psychol.* Feb. 1912.

[2] Cf. Miss C. R. Squire's interesting graduation of the divided rectangle test, etc. *J. of Educ. Psychol.* Sept. 1912.

[3] *Psychol. Clinic,* December 1911.

In conclusion, our experience with the tests leads us to think that with certain changes in their arrangement and with the introduction of certain supplementary tests, the scale can be adapted to Scottish conditions so as to be of great usefulness in the diagnosis of mental insufficiency. One principle we consider to stand in need of special emphasis, the need for conserving the practical usefulness of the scale. Most of the 'revisions and extensions,' based upon an analysis of intelligence, ignore the necessity of a short and simple scheme; the Binet-Simon scale has not been improved upon in this respect by any subsequent one.

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AN INQUIRY INTO SOME QUESTIONS CONNECTED WITH IMAGERY IN DREAMS.

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I. 'Compensation' of imagery.
II. Relation of imagery to amnesia for dreams.
III. Importance and frequency of imagery of different kinds.
IV. External sensory stimuli in dreams.
V. 'Condensation' in pictures and words.
VI. Reasoning in dreams.
VII. Conclusions.

It is the purpose of this paper to consider certain problems which have been suggested mainly by a study of Freud's well-known book, *Die Traumdeutung*. They are:—

1. Does 'compensation' of imagery occur in dreams? That is to say, if a person in waking life is devoid of, or poor in, a particular class of imagery, does it appear in strengthened form in his dreams? And if so, is this a real compensation, or is it merely a case of preference on the part of the dream for the psychically insignificant[1] occurrences which have happened recently[2]?

2. Does the rate at which the dream is forgotten depend on the class of imagery which has been used in it? *e.g.*, if a person's waking imagery is predominantly visual and his auditory imagery poor, while both classes are vivid in dreams, does the auditory imagery vanish first as soon as he wakes?

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[1] Called by Freud 'indifferent' occurrences.

[2] This problem was partly suggested by H. L. Hollingworth's work on "The Psychology of Drowsiness" (*Amer. J. of Psychol.* 1911, XXII. 99–111). He claims there 'compensation' of imagery in the hallucinations of drowsiness. Does such a change in the relative importance of imagery from different sense-organs take place in the dream?

3. What kind of imagery in any particular person usually forms the 'keystone' or central point of his dreams, and is it the class which is most frequently used in waking life and in dreams ?

4. In what percentage of cases in any person is a sensory stimulus detected in the dream on waking and what part has it played in the dream ? Can it be said to have caused the dream ? What kind of sensory stimulus was mostly in evidence and is there any relation between the sense organ involved and the predominant type of imagery in the person ?

5. Do condensations of visual images and of words in dreams occur in different frequency in persons of different type of imagery ?

6. In what degree, if at all, does reasoning occur in dreams ?

I. *'Compensation' of Imagery.*

At this step it is not easy to decide whether 'compensation' of imagery occurs in dreams. For if the question raised in problem 2 should receive an affirmative answer on investigation, *i.e.*, if that imagery which is relatively stronger in dreams be the first to vanish on waking, cases of real compensation must be extremely difficult to discover. For the present, therefore, we will leave this point and examine the material at our disposal with a view to getting some indication as to the validity or otherwise of the notion of compensation.

The dreams[1] of five subjects are available—too small a number, perhaps, to warrant a final conclusion one way or the other, but sufficient to give indications. For convenience we will call the subjects A, B, C, D, E.

A has in waking life a negligible quantity of visual imagery, excellent auditory and motor imagery, good tactual imagery and a small amount of gustatory and olfactory imagery[2].

I have fifteen of her dreams before me. In only four of them is there the slightest trace of visual imagery:—

(*a*) "I saw only a stream of light which changed from one colour to another."

(*b*) "The light on a shiny black belt caught my eye."

(*c*) "I saw a face with two eyes—big and hazel—the most awful eyes I have ever seen. I even noticed the enlarged pupil."

(*d*) "I saw the light on X's spectacles quite distinctly."

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[1] All the dreams were recorded immediately on waking.

[2] A's *dream* imagery is, I think, of considerable psychological importance. Such a case may or may not be rare, but at present little or no notice has been given to the non-visual dream.

In only one dream is there olfactory imagery

"I smelt the bag of soot as he passed me."

No gustatory imagery appears in any of the dreams. Auditory, motor, and tactual imagery are employed by the dreams as vividly as in waking life.

The case of A, therefore, lends no support to the idea of compensation, for the amount of visual imagery in her dreams is no greater than, if indeed as great as, the amount used in waking life.

It is interesting to note that whenever visual imagery does occur with A it is always of light or colour. These have always held a strong appeal for A and colours have, for many reasons, a strong psychic significance for her. It is perhaps therefore natural, that if her dreams are able to employ visual material at all, they should do so in terms of light and colour, rather than in terms of other things that are, psychically, less strongly significant.

B possesses strong auditory and visual imagery in waking life (the latter usually predominant), practically no motor imagery, a fair amount of tactual imagery, and a very little of either gustatory or olfactory imagery.

There are 32 dreams available. In all of these visual and auditory imagery figure equally, exactly as is the case in waking life. In no case has B ever experienced motor imagery in his dreams. Tactual imagery is used occasionally but not so much as in waking life. In only one dream does olfactory imagery appear, and gustatory imagery is never used.

The case of B therefore lends no support to the occurrence of compensation.

C has in waking life strong visual imagery and a large amount of auditory imagery. Motor and tactual imagery are present, but are not strong, whilst olfactory and gustatory imagery are negligibly weak.

There are 80 dreams available. In all except five both visual and auditory imagery occur in much the same proportion as in waking life, *i.e.*, the visual is slightly predominant. Of the five mentioned four contain nothing but visual imagery, and one contains only auditory imagery. Fifteen dreams contain tactual imagery, six contain olfactory, and two gustatory imagery, whilst in two organic sensations are reported. One contains kinaesthetic imagery.

C's is an interesting case. With the exception of the six dreams containing olfactory imagery there is no evidence to support the idea of compensation. C's tactual imagery is strong enough in waking life to warrant its inclusion among the dream material. But with the olfactory imagery it is different: C can only get an olfactory image in waking life with the greatest difficulty, and in most cases not at all. Shortly after C began to record her dreams something was said to her on this point, and her inability to dream smells was commented on. That night she recorded the following:—

> "Almost as soon as I fall asleep I am awakened by a pungent, choking smell of lucifer matches under my nose. I rouse myself in alarm with some difficulty, and sniff, when the smell disappears. I then recognise that this is a distinct effort of the subconscious to dream smells, something having been said the previous day of my inability to do this. I feel enormously pleased."

After this, olfactory imagery appeared at intervals in C's dreams. This looks like a slight effort at compensation, occurring under unusual circumstances, for, before the dream quoted, C has never found olfactory imagery occurring in her dreams. One cannot be certain, however, for in every case some recent indifferent experience was found which might well be the basis of the olfactory image, according to the natural preference of the dream for such incidents. Whether it is a case of real compensation or not, the inclusion among the dream material of a certain type of imagery, following immediately upon the realisation of its absence, is a fact of very great interest.

D possesses intense visual and fair auditory imagery in waking life. His motor imagery and tactual imagery are good; he has a fair amount of gustatory imagery, but practically no olfactory imagery. I possess 25 of his dreams, most of them being extremely long and full of minute detail.

In all of them visual imagery plays the most important part. Auditory imagery is present, but is not good. D several times remarks in his notes that the auditory imagery is not so distinct even as in waking life. Thus he writes, "As I awake someone was saying to me 'This coast is terribly worn away but it is saving itself now by its own sand' (as usual I was conscious of the words, but not of the *sound* of any voice speaking to me)." And again:—"I knew that they were speaking to me although I did not hear the sound of their voices."

D is a very powerful visualiser and in his dreams visual imagery is correspondingly intense. He records (and this is a typical case) "If I had any ability as an artist I could draw her precisely as I saw her in that attitude—every feature is perfectly distinct in my mind." Kinaesthetic imagery is recorded in three dreams, and tactual imagery in only one. No other imagery occurs.

D is a writer by profession, accustomed to visualise his characters "as vividly as though I were watching them in a theatre." His dreams simply continue his waking life so far as imagery is concerned, being exactly like cinematograph films, and there is not the least evidence to support the notion of compensation. Indeed, so far as auditory, tactual and kinaesthetic imagery are concerned, there would seem to be rather a decrease than an increase of intensity.

E has in waking life extremely good visual imagery, but remarkably poor auditory imagery. Tactual and kinaesthetic imagery are good, and gustatory and olfactory imagery are present to some extent.

I have before me 38 of his dreams.

Visual and auditory imagery appear in them all, the former being largely predominant. Kinaesthetic imagery is recorded in only one, and gustatory imagery appears once also. No mention is made of olfactory imagery. Tactual imagery appears once.

E (who is a trained psychologist) says "I hear very clearly in dreams when I hear at all; auditory images in waking life are not strong and are rare." Here then, we have a case which *prima facie* looks very much like an instance of compensation so far as auditory imagery is concerned. On the other hand (as was the case with other subjects) other classes of imagery, *e.g.*, tactual and kinaesthetic, are much less intense in dreams than in waking life.

If this is a case of compensation, why should auditory imagery alone be affected? Possibly because the mechanism Freud calls 'dramatization' demands auditory imagery in preference to, *e.g.*, tactual imagery, as more plastic to its needs; and possibly too because auditory imagery would render easier those mechanisms of 'condensation' and 'displacement' which Freud claims as essential to the construction of the dream, and therefore to the becoming conscious of the latent dream-thoughts. This, of course, is pure supposition and much exhaustive research would be necessary either to substantiate it or to disprove it. A further suggestion, however, may be made, namely, that in normal waking life which necessitates such a vast conscious use of speech and the sense of hearing, auditory impressions would tend to be of far greater psychic significance to E than either tactual or kinaesthetic impressions, and that the inclusion of auditory imagery among the dream material may be due, largely or wholly, to the psychic effects which such impressions carry.

Bringing together all the results we have obtained, we may exhibit them in the following table, shewing the relative values of the subject's imagery in waking life and in dreams:

TABLE I.

Subject	Visual Imagery	Auditory Imagery	Kinaesthetic Imagery	Tactual Imagery	Gustatory Imagery	Olfactory Imagery
(A)						
Waking Life	None	Excellent	Excellent	Good	Poor	Poor
Dreams (15)	None	Excellent	Excellent	Good	None	Poor
(B)						
Waking Life	Excellent	Excellent	None	Fair	Poor	Poor
Dreams (32)	Excellent	Excellent	None	Poor	None	Poor
(C)						
Waking Life	Excellent	Good	Fair	Fair	None	None
Dreams (80)	Excellent	Good	None	Fair	Poor	Fair
(D)						
Waking Life	Excellent	Fair	Good	Fair	Fair	Poor
Dreams (25)	Excellent	Poor	A little	Poor	None	None
(E)						
Waking Life	Excellent	Poor	Good	Good	Poor	Poor
Dreams (38)	Excellent	Good	Poor	Poor	Poor	None

We have 'dissected' 190 dreams, the product of five subjects, from the point of view of the imagery they contained, with a view to gaining some indication of the validity or otherwise of the notion of compensation. 20 % of these dreams give evidence of what may be compensation; the remaining 80 % fail entirely to bear out such a theory. No dogmatic conclusion can fairly be drawn owing to the small amount of material before us; but from such data as we have been able to gather, we may conclude that the weight of evidence is not in support of the occurrence of compensation[1].

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[1] It may be noted that these results do not agree in the main with those of Hacker (*Arch. f. d. ges. Psychol.* 1911, XXI. 16), who finds that as regards their frequency of appearance in the dream, the images corresponding to the different sense-regions are in direct ratio to the frequency of the sensations corresponding to them in waking life, and that if a person belongs to a definite image type, it is not necessarily that imagery which appears in the dream with the greatest frequency. Thus, in 100 dreams Hacker records visual images in 93, auditory images in 73, kinaesthetic in 18, tactual in 16, and gustatory and olfactory in 3. These numbers agree broadly with those of Weed and Hallam (*Amer. J. of Psychol.* 1896, VII. No. 3) and of Calkins (*ibid.* 1893, V.). Our results seem to point to a different conclusion, which seems *a priori* more probable, namely that that class of imagery is most frequent in dreams which is most frequently employed in waking life.

II. *Relation of Imagery to Amnesia for Dreams.*

From the nature of the problem introspection is the only means of its solution, and the data at our disposal comes from B, C and E, who have systematically recorded their dreams for a considerable time.

B, whenever he is able to remember a dream in full detail, recalls with great exactness the visual elements and the actions (these being recalled in terms of 'secondary' visual imagery, for he possesses no kinaesthetic imagery). The auditory elements, which enter largely into all his dreams, are in most cases only partially recalled. Often they vanish completely on waking; nothing remains but an 'awareness' that auditory imagery has been employed, imagery which the most persistent efforts fail to recall. The auditory imagery invariably vanishes first; B never remembers having recorded a dream in which the recalled auditory imagery was as complete as in the dream itself.

In those dreams which have been extremely elusive and which have persisted for only a very short time on waking, it has always been found that the visual elements had the greater perseverative tendency, the auditory vanishing almost instantaneously.

Much the same occurs in the case of C so far as auditory and visual imagery are concerned. The visual elements tend to persist, while the auditory imagery is more fleeting and rapidly fades away. Here, too, dreams are often remembered vividly in their visual details while nothing but an 'awareness' of auditory imagery remains. C's tactual imagery however, when it occurs in dreams (as it did in about 18 %), tends to persist to an extraordinary extent, often lasting with the vividness of reality long after both the visual and auditory material has become faint or vanished altogether[1]. I may perhaps cite one particular case. C records, "I take from a cord above a long table a huge hank of brown American toffee, and scraping my thumb nail in it write on the white table. It is lovely scrolled writing. I feel the toffee on my hands and get rid of it all except an astringent feeling round the nails which will not go." When, some hours after waking, C handed the dream to me, she remarked that she could still feel the toffee clinging to her fingers as vividly as though it were really there. By that time the rest of the dream had become very hazy and indistinct.

E reports much the same phenomena. As we have seen, his dreams work for the most part with visual and auditory imagery. He finds that the auditory elements tend to fade away before the visual, being forgotten with much greater rapidity. (He says " they are the parts of my dreams which vanish almost at once as I awake and unless I fix them by saying them, they vanish irrecoverably.")

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[1] A has often told me of similar cases in her own experience, where long after the rest of the dream had faded the tactual imagery persisted with intense vividness, often for many hours, and sometimes for a whole day or more.

B has studied his dreams during a period of about nine months, C for about six months, and E during a period of about eighteen months. Their evidence therefore carries some weight, and one may fairly draw some conclusions from it.

The rate at which dreams are forgotten seems to depend to a large extent on the class of imagery employed in them, the type of imagery employed most naturally in waking life seeming to have the strongest perseverative tendency. B, C and E are all strong visualisers in waking life, and the visual imagery of their dreams is the class that persists longest. Auditory imagery is quite secondary in waking life with all three (and especially so with E), and the auditory imagery of their dreams has a much less perseverative tendency than the visual.

We have noted above the intimate connexion of this problem of the rate of forgetting with the problem of compensation. The rate of forgetting has been found to depend in part upon the relative values in waking life of the imagery employed in the dream. This is an added reason why one could only indicate where the weight of evidence pointed in the previous problem, without drawing any rigid conclusion. It may well be that cases of compensation occurred in other senses which, owing to the unstable and fleeting character of the dream imagery, were thereby withheld from recognition.

The results of this part of our investigation may be conveniently summed up in the following table:—

TABLE II.

Subject	Value-order of waking imagery	Relative order in which dream imagery is forgotten
B (9 mos. observation)	(1) Visual (2) Auditory	(1) Auditory (2) Visual
C (6 mos. observation)	(1) Visual (2) Auditory (3) Tactual	(1) Auditory (2) Visual (3) Tactual
E (18 mos. observation)	(1) Visual (2) Auditory	(1) Auditory (2) Visual

III. *Importance and Frequency of Imagery of Different Kinds.*

It has frequently been difficult to decide which class of imagery ought to be held of highest importance. In such cases, where, upon psycho-analysis, two classes of imagery both give rise to highly significant latent dream thoughts, that class was chosen which, in the opinion of the subject, gave rise to the most significant latent dream thoughts. The data used were the dreams of A, B, C and D.

In each case the central *motif* of the majority of their dreams lay in that class of imagery which in dreaming and waking life was most frequently employed. The proportion was as follows:—

(1) A: 15 dreams: auditory 10; visual 1; tactual 4.
(2) B: 32 dreams: visual 23; auditory 8; olfactory 1.
(3) C: 80 dreams: visual 53; auditory 24; olfactory 2; tactual 1.
(4) D: 25 dreams: visual 24; auditory 1.

The following table sums up the results:—

TABLE III.

	PROPORTION OF IMAGERY CARRYING THE CENTRAL *motif*					
Subject	Visual	Auditory	Tactual	Motor	Olfactory	Gustatory
A (15 dreams)	7 %	66 %	27 %	0	0	0
B (32 dreams)	72 %	25 %	0	0	3 %	0
C (80 dreams)	66 %	30 %	1 %	0	3 %	0
D (25 dreams)	96 %	4 %	0	0	0	0

IV. *External Sensory Stimuli in Dreams.*

An examination of the dreams of A, B, C, D and E yields the following results:—

Two of A's dreams contain material apparently produced by an external stimulus:—

(*a*) Before going to bed she tied a string tightly round her right foot just above the ankle. In the course of a long dream she records: "I was handed a jug which I could feel was smooth and shiny. This I put into the sea....I put one foot into the jug and then balanced myself with one foot in the air."

(*b*) She records at the end of a long dream full of detail, "I took off the coat and went into another room: I started wiping dishes....I dropped one which broke with a crash. I awoke." When she awoke she found that the wind had blown down a picture which lay in bits on the floor.

In neither case could the stimulus be said to have 'caused' the dream in any way whatever. In (*b*), especially, it seems to have been worked into the dream simply because it was of a nature which enabled it to be suitably united to the presentation content of the dream.

In B's dreams, a sensory stimulus was detected on waking in two cases:—

(*a*) He records "...the candlestick toppled over the edge and fell: I tried to catch it on my right foot and it fell on my shin heavily, breaking the bone and giving me most exquisite pain." B was then driven to the doctor's in a motor car. He goes on—"I was sitting next to the driver with my leg sticking out over the bonnet of the car; it felt horribly cold." On waking, B found that the bedclothes had slipped off, and that his right foot had become wedged in an uncomfortable position between the bed and the wall.

(*b*) At the end of a long dream he records "I proceeded to give the class a geography lesson. In the middle of it the blackboard fell down with a fearful clatter and several children rolled off their seats with much noise. I awoke to find the 'boots' hammering on my door to awaken me."

Again in neither case did the stimulus 'cause' the dream. As in A's example, (*b*) would seem to have been worked in because it was of an acceptable nature to the tenor of the dream.

In four of C's dreams an external stimulus was detected upon waking, and in one the presence of an organic sensation was noted.

C records (all very long dreams)—(*a*) "...someone says someone else is sending me his donkey. I wish it was a dog, but think I will try to make the best of it. The donkey comes, and I am agreeably surprised to find it is Dash, a curly brown spaniel I know. I cuddle him and feel his soft curls under my chin (when I awake I find a rug which I have thrown on the bed is smothering my neck)."

(*b*) The end of a very long dream. "...I hear a noise of some one talking like a corncrake and waken startled, to hear a spaniel barking in the street."

(*c*) "...I say 'there is a teapot full; it is a pity to waste it.' I feel the teapot in my hand—it is very hot." C woke later to find her hands over a hot water bottle.

(*d*) "...I am walking down — street. An undergarment is unbuttoned and I am frantically afraid it will slip off. I clutch my skirts with both hands and begin to hurry....(I awake to find the bedclothes have slipped and that my legs are very cold)."

In each case the stimulus accounts for only one element in the dream—it cannot be said to be the 'cause' of it.

D detected a physical stimulus in two cases on waking. He records (during two very long dreams):—

(*a*) "The dream ended by my making an excuse to leave the room. I wanted to evacuate, and the W.C. was next door to the drawing room. I saw that the door of the W.C. was standing open and closed it—intending to go to the privy outside the house—and then thought that the people in the drawing room would have heard me close the door and that I must show myself again at once in the drawing room. I went back to the drawing room and was detained by someone who spoke to me although I was now most anxious to get away. (This is one of the dreams that I almost invariably suffer in some form or another when I am at all troubled by constipation, as I was last night. Usually, however, it takes a form of much greater unpleasantness, with sight of, or even contact with, the fæces.)"

(*b*) "My wife and I were in bed: the window alongside the bed was open at the top and there was a whole swarm of bees in the room. I was nervous about these bees. I heard the sound of their humming and feared lest they might swarm on us while we were asleep. (When I awoke I placed the sound of the humming as that made by the single telegraph wire which runs within a few yards of our window. The really interesting thing in connexion with the auditory stimulus of the telegraph wire is that although I have slept in the same room, under the same conditions for over four months, this is the first instance of that stimulus affecting me.)"

These are two interesting examples. In (*a*) the stimulus appears to have been transformed into action appropriate to the stimulus (the stimulus itself being reproduced). In (*b*) the supporters of a 'stimulus' theory would find it hard to explain why for four months D's dreams should persistently ignore the stimulus, and finally reproduce it in a disguised form.

In the 38 dreams of E that I possess there are two cases in which an external stimulus is detected upon waking:—

(*a*) E records in the middle of an extremely long dream, "I felt it unsafe to go by ferry, but as the boat comes I prepare to go down the steps, but a man upbraids me because I am pulling up my trousers (which have slipped) in view of the other people. I adjust them and prepare to rush for the ferry....(As I awoke, I found that the string of my pyjamas had slackened and my trousers were actually about my hips instead of round my waist. Note the relative unimportance of the bodily stimulus in the dream. I did not feel in the least embarrassed as I readjusted my trousers in the dream—it was a mere incident.)"

(*b*) A feeling of discomfort gives rise to a 'lavatory' incident in the dream similar to the example of D's quoted above.

In neither of these cases could the stimulus possibly have caused the dream. In (*a*) especially it was the merest incident in a long and involved dream of adventure and dispatch carrying.

External stimuli then appear to play but little part in the formation of dreams. Where they have been detected they explained only one element of the dream—and in no case is it a vitally important element. It is noteworthy that while the nature of such stimuli—either tactual or auditory—seems to bear no relation to the predominant imagery of waking life, touch and hearing are the two spheres of sensation that during sleep are most easily accessible to chance stimuli.

Our investigations have borne out some of Freud's conclusions, namely, (i) that only those stimuli are used by the dream which admit of easy unification with the presentation content; (ii) that such stimuli are misinterpreted for a very definite purpose and by a very definite mechanism, *i.e.*, by the action of the restraining psychic agent which Freud terms 'censorship.'

The following table summarises the above results:—

TABLE IV.

Subject	Percentage of cases in which a stimulus was detected	Nature of stimulus	Predominant type of imagery in waking life
A (15 dreams)	13 %	Tactual (1) Auditory (1)	Auditory
B (32 dreams)	6 %	Tactual (1) Auditory (1)	Visual
C (80 dreams)	5 %	Tactual (3) Auditory (1)	Visual
D (25 dreams)	8 %	Tactual (1) Auditory (1)	Visual
E (38 dreams)	5 %	Tactual (2)	Visual

V. '*Condensation*' *in Pictures and Words.*

'Condensation' in Freud's sense of the word means two slightly different things which it is well to distinguish. (*a*) The dream employs condensation in the sense that every element in the manifest content is 'over-determined,' *i.e.*, is the representation of *several* dream-thoughts. "The dream is reserved, paltry, and laconic, when compared with the range and copiousness of the dream-thoughts....the amount of condensation is—strictly speaking—indeterminable[1]." (*b*) The dream employs condensation in the sense of forming composite images of things, persons, or words, by the fusion of two or more into a coherent whole. While condensation in the first sense of the word must play a part in the formation of every dream, the second type of condensation need not necessarily be present.

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[1] *Interpretation of Dreams* (Eng. trans.), p. 261

It is this latter type of condensation that concerns us, and our immediate problem is to see whether the frequency of such condensations varies according as they occur with visual or auditory material, and whether their frequency is related to, or affected by, the predominant imagery of the subject.

Our material is again the dreams of the five subjects A, B, C, D, E.

In B condensations of pictures appear in 16 dreams, and condensations of words in three.

In C condensations of pictures appear in 53 dreams and condensations of words in five.

In D condensations of pictures appear in 16 dreams, and condensations of words in one.

In E condensations of pictures appear in 28 dreams, and condensations of words in four.

A's case is of more interest than the others. A does not visualise in dreams and consequently no condensations of visual images are found. Condensations of words appear in three dreams.

In three of A's dreams condensations appear to occur which are of extreme interest. A records:—

(*a*) "I was walking down a road between two people—X and Y—the one on my right spoke and I recognised X's voice "Shall we go into George Street, Y?" The person on my left (Y) answered, but the voice I have never heard before—I can't place it anywhere: it had a curious note in it which I should recognise at once."

Here we have the awareness of a person (Y was not visualised) combined with the voice either (*a*) of a second person, or (*b*) of two people combined. Observe that A remarks on the "curious note" of the voice. It was a type of voice that she was certain had never come within her experience, and it leads one to suspect that a condensation of two voices may possibly have taken place.

(*b*) "I went upstairs, to find the person sitting down crying and sobbing. She spoke to me and asked me to take her downstairs, saying she could do nothing. I did not know the voice, which was like my sister L's, only with a curious squeak in it like M's."

Again we have what looks like an attempt on the part of the dream to effect a condensation of two voices.

(*c*) "I was sitting in a room with a number of people whom I did not know. The person next to me whispered in X's voice, "O, I am so frightened; is it the Germans coming?"

Here we have the awareness of one person combined with the voice of a second person.

These results, which are summarised in Table V, seem to shew that condensations are more frequent in visual material than in auditory material, and that condensations occur most frequently in that kind of material which in waking life corresponds to the predominant imagery of the subject.

Table V.

Subject	No. of dreams	No. of dreams containing visual condensations	Percentage of dreams containing visual condensations	No. of dreams containing auditory condensations	Percentage of dreams containing auditory condensations
A	15	0	0	6	40 %
B	32	16	50 %	3	9 %
C	80	53	66 %	5	6 %
D	25	16	64 %	1	4 %
E	38	28	74 %	4	11 %

VI. *Reasoning in Dreams.*

So far as I have been able to discover, Freud only offers two remarks on the subject of reasoning in dreams, and these remarks are entirely opposed. He says:—

(*a*) "Vielleicht wird sich hier ein Einwand erheben, der für den Traum den Verzicht auf die Darstellung logischer Relationen bestreitet. Es gibt ja Träume, in welchen die kompliziertesten Geistesoperationen vor sich gehen, begründet und widersprochen, gewitzelt und verglichen wird wie im wachen Denken. Allein auch hier trügt der Schein; wenn man auf die Deutung solcher Träume eingeht, erfährt man, dass dies alles *Traummaterial ist, nicht Darstellung intellektueller Arbeit im Traume*. Der *Inhalt* der Traumgedanken ist durch das scheinbare Denken des Traumes wiedergegeben, nicht die *Beziehungen der Traumgedanken zueinander,* in deren Festsellungen das Denken besteht....Am leichtesten ist es aber zu konstatieren, dass alle Reden, die in Träumen vorkommen und die ausdrücklich als solche bezeichnet werden, unveränderte oder nur wenig modifizierte Nachbildungen von Reden sind, die sich ebenso in den Erinnerungen des Traummaterials vorfinden[1]."

(*b*) "I shall not deny, indeed, that there is also critical thought activity which does not merely repeat material from the dream thoughts and which takes part in the formation of the dream[2]."

On p. 283 he says "In the case of a young man who was suffering from obsessions but whose intellectual functions were intact and highly developed, I recently found the only exception to this rule." See (*a*) above. "The speeches which occurred in his dreams did not originate in speeches which he had heard or which he had made himself, but corresponded to the disfigured wording of his obsessive thoughts, which only came to his consciousness in a changed state while he was awake.

Miss Calkins[3] came to the conclusion that real intellectual activity did take place in dreams. She says, "Not only imagination but real thought occurs in dreams though the fact is often denied[4]. The frequency of conversation in dreams might be looked upon as a proof of the existence of the lower stages, conception and judgment, of thought activity, for both are necessary to the use and combination of words. But such dreams may be instances of merely visual or auditory memory of words, and if there were no instances of explicit judgment and of reasoning in dreams the presence of thought could hardly be proved. But not only do we form judgments in our dreams sometimes correctly though often incorrectly, but we carry on whole trains of reasoning, reasoning sometimes incorrectly, sometimes correctly, from premises—and less often both correctly and from natural premises."

Here we have two opposed results—on the one hand that reasoning does occur in dreams, on the other hand that it does not. I propose to take the dreams of subject C, and endeavour by their means to shew that the results obtained by Miss Calkins are correct, *i.e.*, that 'critical thought' is active in dreams.

Of the 80 dreams C has recorded, nine shew good evidence of such thought activity. The essential parts of the dreams are as follows:—

(*a*) "I am in the old sitting room at home. Before me stands an exquisitely beautiful woman gowned in a soft green robe....I gaze for what seems quite a long time and then realize that for all her calmness she is in some awful grief. I long to comfort her and try to take her in my arms. Then I suddenly realize who she is—it is Shakespere's Ophelia! I cry out 'Oh, Ophelia, do not die! I will love and comfort you, I will indeed!' she stares at me, unseeing, and walks away. I then think she is very unlike any former Ophelia I have read about or thought of, and I am still puzzling over it when I awake."

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[1] *Die Traumdeuiung*, 3 Aufl., Leipzig, 1911, 246–7.

[2] *Interpretation of Dreams* (Eng. trans.) p. 291.

[3] *Amer. J. of Psychol.* 1893, v. 325.

[4] Freud's followers, especially Jones, are most dogmatic on this point.

(*b*) "He hands me a book of words saying, 'can you learn that off very fast?' I take the small brown paper book and read a sentence: it runs 'as for the performances in music halls they are all alike bad.' I declaim this and then say 'I suppose if I can't remember all this, clever words of my own will do?' He says 'you must give the cue.' I reflect 'the cue is "bad," I'll remember that.' He says 'you'll be lowered to the stage in a wire cage: mind you kick about well as though you were in agony, that'll help it.' He turns to someone and says 'will no one fetch the book she actually has to learn?' I stand reflecting that he must mean a book in which only my own words are written, and how awkward this will be for me when I do not know the run of the play. While I am thus reflecting and watching the door through which the attendant has disappeared for the book, I awake."

(*c*) "A sudden gleam of commonsense comes to me, and I turn and face poor E. I take her hand and pat it saying, 'compose yourself, my dear, you shan't be buried: no one on earth has power to bury you while you are still alive.' E stops screaming and looks at me with dawning hope. Someone says angrily 'she'll sing a different tune in the morning!' I clasp E's hand closely, frantic for fear she should hear they are determined to bury her. She sobs 'I'm so frightened.' I say 'X and I will stay with you all night, won't we X?' To my sorrow X looks doubtful and as though he thinks she ought to go in the coffin quietly. I struggle to say (I have the whole sentence clear cut in my mind before I begin) 'Listen! Until you are dead no one can bury you. If your spirit ever leaves your body it will be already with your Heavenly Father, and you would be too happy to care what they do with your body.' To my grief I cannot get this out....I awake."

(*d*) "I am in a long corridor train: it is brilliantly lighted with electricity and full of continental passengers...as I do not know anyone I lean back and think. I feel the train rock from side to side with a long swaying motion, but do not hear the muffled rumble usual to a train in a tunnel. Where am I going alone in this way? What are we tunnelled under? How if the roof caved in? The death would probably be swift, so no matter. I try to imagine the squash. Hurrah! mountains so heavy and long must mean Switzerland! Thoughts of the beauty waiting at the other end of the tunnel conquer all uneasiness. I am standing out on a wayside station in cold sweet mountain air."

(*e*) "The duke and agent are still there, also some ladies and a noble lord. I know he is a noble lord because he is the exact reproduction of the comic-opera variety, the face of a *roué*, high-bridged nose, and fur-lined overcoat. I entertain the company with some specimens of Yorkshire dialect. This amuses the noble lord so much that he rocks with laughter and knocks up against me. I jump up theatrically, and feel my breast pocket: my purse is gone! The noble lord hands it to me with a low bow 'you have dropped it, Madam.' I open it and find the coins correct." (A curious piece of brain-working in the dream now comes in.) "I know I am dreaming and think 'why, I am dreaming the plot of a story!' *viz.*: The lady took her purse, and thanking the noble lord in a low voice, examined the coins. Her suspicions were incorrect, all was in order. It was not until she came to try and spend the coins that she found false coins exactly corresponding to the amount she possessed had been deftly substituted in her purse. I make a mental note to use this brilliant idea.....I look at the Bentham bridge and see straddling across the railway lines the towers of Dublin Castle. Entranced by the architecture I walk through the gateway and find myself in the track of an on-coming express. I draw back hastily, and think it a silly place for a castle."

(*f*) "I get up at two o'clock in the morning to set the third instalment of the serial story I am writing on the typewriter. I carry up the clothes-horse and the step-ladder to prevent the noise of the machine annoying E; while I am struggling with them I suddenly think 'why not shut the door ?' While I am typing I realize I could put a new instalment of the story straight on to the typewriter. I begin the first sentences. (I have now forgotten these, but still retain the idea; and I am interested to note that it is really serviceable and clears up a difficulty of plot construction at which I had previously worked and abandoned as useless.)"

(*g*) "I am rushing down to the central stores with a large market basket in one hand and waving a cheque sheet dramatically in the other. I see the projecting clock very clearly: large brass pointers shew 5 [minutes] to 12. I stand and consider 'can I get up to the bank and down to the central grocery all in five minutes, for they close prompt at 12.0 ?' I reflect that if I am in the shop before it strikes they will be obliged to wait upon me. I think I will make a dash for it. The thought comes 'what if there is a line of people waiting at the bank counter ?' Just then the brass pointer slips on three minutes, and I decide the cheques must wait."

(*h*) "I am in a torpedo-shaped car sitting beside X who is driving....I reflect that X knows nothing whatever of driving motors and will probably spill me out. I do not feel fear, only uneasiness: then I reflect, 'It will in any case be quickly over and as good a way out of my worries as any.' I am wondering whether I shall pitch on my head, in which case I should die instantly, when I suddenly see a sharp brick wall projecting into the road. I think 'now for it,' but the car glides round with a beautiful curve."

(*i*) "I was in the large meeting house: several persons gave addresses which I regarded as futile, though I did not gather what they said. I got up, and walked in a very impressive manner to the steps of the minister's gallery and proceeded to discourse as follows—'There is no disgrace in being poor. Jesus was poor. He ate bread, not cake. (I think this makes a profound impression.) He dressed poorly. "Behold those who wear soft raiment are in King's palaces." Those who eat must work. Jesus worked for all he ate. (I pause, remembering that Jesus was probably the recipient of much charity, recover, and go on.) He went up to the feast at Jerusalem. He fed the five thousand.' (I pause, horribly conscious that he was not at Jerusalem when he fed the five thousand. What shall I do ? How shall I get out of it ? Reason suggests—turn it into a talk about gathering up the broken fragments of life, and love, and opportunity, like you gave the A— women tonight. Commonsense says, that sort of talk isn't at all suitable for this meeting. I am still standing, considering mentally, the Friends waiting on my words with flattering and exaggerated attention, when I awake extremely glad to find it is only a dream.)"

These dreams need little comment: they are, it seems to me, good evidence that in dreams intellectual operations, judgment, criticism, comparison and the rest, can take place as readily as in waking life. In the dream last quoted especially, careful psycho-analysis entirely failed to elicit anything that could relate the trains of reasoning to the events of waking life and I am satisfied that it is conclusive evidence of the reality of reasoning in dreams. I do not claim indeed that such intellectual operations take place in all, or even in the majority of dreams. Only 11 % of C's dreams shew evidence of such mental processes to any appreciable extent. But the dreams above quoted seem to warrant the conclusion that reasoning, with all its niceties of logical connexion and opposition, can take place in dreams as readily as in waking life. If a dream element is connected with the events or thoughts of waking life, psycho-analysis, if pursued far enough, claims inevitably to reveal that connexion: where such psycho-analysis entirely fails to account for dream elements, it assumes that such elements are not memories transferred from the waking state but are materials evolved from the inner consciousness by the creative activity of the dream itself. That the dream can actually create is proved indisputably by example (*f*) above. The difficulty of plot construction was entirely cleared up in the dream and the idea provided was actually used later by C in her work. But example (*i*) provides us with the finest example of reasoning in dreams. Here psycho-analysis left no doubt that the train of logical thought was the creation of the dream and not a case of mere (even modified) memory.

VII. *Conclusions.*

1. The weight of evidence is opposed to any notion of 'compensation' of imagery in the dream; the dream tends to reproduce the relative proportion of the imagery of waking life. One slight exception to this was found in the auditory imagery of E's dreams.

2. The rate at which the dream is forgotten depends partly upon the type of imagery used in it. The dream imagery tends to be forgotten in the inverse order of the relation—value of waking imagery.

3. The imagery carrying the central *motif* of the dream tends to be that imagery which predominates in the waking life of the subject.

4. Sensory stimuli do not cause the dream, and play only a small part in its formation, accounting as a rule for only one element of the presentation content. They are worked into the texture of the dream

only if they prove capable of being easily united with the presentation content. They suffer distortion at the hands of the 'censorship' and consequent misinterpretation by the receiving mind. The sense organ involved bears no relation to the predominant imagery of the individual.

5. Condensations occur more frequently in visual than in auditory material; in auditory elements condensations occur more frequently with words than with sounds. Condensations occur with the greatest frequency in the material which forms the predominant imagery of the subject.

6. 'Critical thought' and reasoning occur in dreams, though not frequently; where they do occur they shew all the clearness and logical consistency of waking trains of thought.

In the course of this investigation I have gathered ample evidence from time to time of the truth of many of Freud's statements. The mechanisms of Condensation, Displacement, and Dramatization have often appeared in the course of the psycho-analyses. On the other hand, I have been led to doubt the universal validity of others of his statements, notably that every dream contains sexual and infantile material, and that every dream derives its motor force from an unconscious wish[1].

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[1] I am aware that the argument will probably be advanced that, had the psycho-analyses been carried far enough, the sexual, the infantile, and the unconscious would inevitably have appeared. To this I have two answers. First, that as Freud himself says, one can never be certain that a psycho-analysis has come to a complete finish; hence when, without these factors, an interpretation of every dream element is obtained, it is superfluous to proceed further in an endless search for a hypothetical basis which is not needed to interpret the dream satisfactorily; secondly, that Freud's own analyses, especially in the first three long dreams in *Die Traumdeutung*, give no proof of the universal appearance of the sexual, the infantile, or the unconscious.

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IMMEDIATE MEMORY AND ITS EVALUATION.

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I. Introduction.
II. Previous work on the evaluation of immediate memory.
III. The span of prehension.
IV. Errors of partial reproduction.
V. Impression and expression.
VI. Conclusions.

I. INTRODUCTION.

In 1910–11 a series of experiments, among children of both sexes, was carried out by the writer in one of the *Bezirkschulen* at Leipzig[1], on the relation of intelligence to learning and remembering. Whilst broadly demonstrating that immediate memory is better in intelligent than in unintelligent children, these experiments at the same time indicated that this form of memory is of a much more complex nature than has been hitherto supposed. With a view to the further investigation of the nature of immediate memory and its different manifestations in varying degrees of intelligence, a fresh series of experiments was undertaken in the February and March of 1912 upon children from a South Wales school[2]. Experiments were also carried out upon four adults (two men and two women), all elementary school teachers, who had had no previous experience of experimental work, and, therefore, might be expected to be confronted with the same primary difficulties of learning as the children.

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[1] See "Beziehungen zwischen der Intelligenz und dem Lernen und Behalten," *Päd.-Psychol. Arbeiten*, II. 65.

[2] I am indebted to the Head-teachers and Staffs of the Dowlais Elementary Schools, who not only readily gave me permission to conduct my experiments, but also afforded me much valuable help in carrying them out.

The children were 20 in number, ten boys and ten girls, and were taken from classes of 50 boys and girls respectively, whose average age amounted to ten years. The boys and girls had been divided by the class teachers into two groups, one containing intelligent, and the other backward children, the standard of general school work being taken as the basis of classification. The children were chosen in equal numbers from these two groups, but care was taken not to choose the very best children, so as to avoid, if possible, the precociously developed child; whilst those selected at the opposite extreme were, as far as was known, free from physical or mental defect.

In the case of the boys, the work was carried out in the afternoon, in a private room at the school, and with no one present except the experimenter and the subject. The girls were tested in the same fashion in the morning. The work with the adults was done under similar conditions, that with the men in the evening, with the women in the morning. The material adopted for use in the experiments consisted of numerals (single digits) and senseless syllables.

The numerals were arranged in one group of five series, each series consisting of nine rows containing respectively from two to ten units. The syllables were similarly arranged in two groups of five series, each series consisting of seven rows containing respectively from two to eight units[1]. The numbers were written, and the syllables typed, in vertical columns on unruled sheets of foolscap paper. This paper was enclosed in a folding sheet of thin blue cardboard, in which nine slots of varying length were cut, each slot being adapted to the length of a row. A similar sheet of cardboard containing a slot cut to fit the width of the row, and narrow enough to admit of the exposure of just one syllable or number at a time, was used by the subject. This was placed over the slot exposing the row of two units. The subject was then required to slide the sheet regularly down the row, repeating each unit aloud as it was exposed[2]. As soon as the last unit was read, the row was immediately repeated 'by heart,' in the same order, beginning, as far as was possible, with the first. No instructions, for obvious reasons, were given to avoid sensible associations, though, when they occurred, the subject was told not to rely on such associations as a means of reproduction. Sufficient time was given—varying with the rapidity in answering—to allow of efforts of recall. The sheet was then placed in position above the second row and the same procedure followed. The subject was informed of the length and nature of each row before it was exposed.

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[1] In the series arranged for the adults, from two to ten units.

[2] It was thought advisable to let the child, rather than the experimenter, move the sheet. By this 'motor adjustment' the child was guarded from that state of mere passive expectancy which works so unfavourably when mechanical apparatus is used.

Some difficulty was experienced in choosing the mode of presenting the rows. Previous work on the subject does not point to any very definite conclusions as to the most satisfactory form of exposure. The weight of opinion seems to be in favour of the visual exposure of the rows, particularly of syllables. Binet[1] inclined to the view that "l'expérience a appris qu'une multiplicité de sensations favorise la mémoire." Watt[2] on the other hand writes that "there is not the slightest use in multiplying forms of imagery in the process of learning." As my main purpose was to investigate differences in the learning process, it was thought advisable to let the subjects learn the rows under conditions as similar as possible to those under which their usual school work was performed. They were, therefore, permitted both to see the material and to read it aloud. The rate of reading was left to the individual, care being taken, of course, that the rate was not so slow as to allow of any revisional repetition of individual members before the whole row had been exposed. It was found that the children quickly adapted themselves to a rate of reading which tended to remain fairly constant, though it naturally varied considerably with different individuals. One exception had, however, to be made. The reading of the syllables was found to present considerable, if not an insuperable, difficulty to the more backward children. These rows were therefore read aloud by the experimenter, as well as by these children. This procedure, naturally, had obvious dangers. It was, however, decided on for the following reasons. In the first place, one child persisted in reading the syllables given in the preliminary test in a wrong form, though on reproducing them she apparently—in the rows of two units at least—relied mainly upon her acoustic imagery, and gave the syllables correctly. Again, rows and even single syllables previously exposed had, especially with backward children, considerable influence upon a freshly exposed row. In consequence, the row, either in whole or in part, was often not really apperceived, but rather assimilated to the persistent image. Such is the influence of this persistence that though the word was read properly to the child, yet some combination of it with the persistent image (*e.g.*, see below, pp. 343 and 344) would often be read, and though the error was at once corrected by the child, it would afterwards be reproduced in combinations showing little variation.

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[1] *Les idées modernes sur les enfants*, 122.

[2] *The Economy and Training of Memory*, 101. Pohlmann (*Exper. Beiträge zur Lehre vom Gedächtnis*, 184–5) found, as one might expect, that syllables were better learnt visually and words orally. Opinions, however, differ on this point; see Hawkins, "Experiments on Mental Types," *Psychol. Rev.* 1897 and Meumann, *Ökonomie und Technik des Gedächtnisses*, 129.

As will have been gathered, the rows were reproduced orally. This plan was adopted in preference to the method of writing them down. In rows near the threshold of immediate memory especially, it is essential that they should be repeated immediately; otherwise the rapidly fading impressions are not capable of repetition. In such cases their reproduction in writing presents serious difficulties, for the act of writing inhibits the power of repetition, driving away the ever weakening impressions.

At the first sitting, when the numbers were given, a preliminary test of merely reading a row of numbers, and then of reading and reproducing rows of from two to five numbers was taken. This test was intended to give practice in the use of the cardboard sheet. Similarly, at the second sitting, a row of ten syllables was read through, and a series of three rows of from two to four units was read and reproduced. In the succeeding sittings, as each series began with a row of two units, no preliminary test was given, the repetition of this short row being regarded as sufficient preliminary exercise. The whole of the experiments comprised three sittings, one for the numbers, and one for each group of syllables.

II. PREVIOUS WORK ON THE EVALUATION OF IMMEDIATE MEMORY.

Before we proceed to a description of the results of our experiments, some review of previous work on immediate memory is necessary. It is now generally accepted that the intelligent child gives evidence of a better immediate memory than the backward, and a much better general memory than the weak-minded child. The methods upon which such conclusions are based have not, however, been as yet subjected to any standardised evaluation. Most probably, this has been due to the emphasis laid on the value of the test as one of a series[1], where each test is intended mainly to produce a certain general estimate.

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[1] As in the Binet tests.

The usual method adopted is, therefore, to combine the results of any test of immediate memory. The numerical expression of these results is then regarded as evidence of intellectual grading. It is obvious that investigations of this nature depend upon quantitative rather than upon qualitative values. Thus, little stress has been laid on the functional differences which, especially in the case of immediate memory, play so important a *rôle*—differences, moreover, which may often modify to no small extent the value of the numerical results thus obtained. A glance at the history of the methods employed in the estimation of immediate memory will bring this out more clearly.

The method first adopted was that in which the given reproductions were taken *en masse* and estimated as a percentage of the total number of members in the whole of the rows presented. Jacobs[1], in his experiments with syllables, numbers and letters on the span of prehension among adults and school children, took apparently only those rows which were given correctly, making no allowance for rows beyond the threshold, where misplacement either of digit, or of vowel, or consonant sound may have taken place. In this manner, a somewhat vague comparative estimate was obtained of the variations in immediate memory among children of varying grades. But it is clear that such a method, when adopted as a general estimate, is open to grave objection. Important matters, such as the order in which the units of the row are reproduced, to say nothing of individual differences in the mode of reproduction, are entirely passed over. Moreover, the many cases where the syllables are but partially reproduced (cases which provide material for the study of the most interesting of the differences that occur) are neglected, and are not given even partial values in the final estimate.

The next plan—one borrowed from methods employed in the investigation of mediate memory—was to take into account the manner in which the rows were reproduced. Thus the reproduction of a row of six syllables in their right order has a value quite different from that of a similar row the whole six units of which are repeated in the wrong order. To avoid such difficulties, a graded system of marking was adopted, and for this purpose two methods were devised[2]. In the first, or negative, method, marks were deducted for any failures committed. A numeral or letter omitted, for example, or one inserted which had no place in the row, caused the loss of one mark, whilst errors of misplacement were recognised by the loss of half a mark. In the second, or positive, method, marks were awarded in the following manner:—(*a*) for each unit named in the wrong position, or when the position was not known, one mark was given, (*b*) for each unit reproduced with imperfect knowledge of its position, two marks were given, whilst, (*c*) if the units were reproduced correctly, it was given three marks[1].

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[1] "Experiments on Prehension," *Mind*, 1887, XII. 75.

[2] Cf. Smith, "Relation of Attention to Memory," *Mind*, 1895, IV. 51.

Here, then, each unit of the row is given a fairly definite place value, based on the assumption that to reproduce a row in the right order implies a better 'memory' of the row than is the case when it is reproduced in the wrong order. Such an assumption is at best but an arbitrary assignment of values, and is based apparently on the doubtful supposition that each individual member of a given row has an equal reproductive place value, irrespective of its position in the row. A glance, however, at the curves which plot the relative rate of forgetting[2] of the units in any given series of rows, shows the inherent weakness of this assumption. For, as is to be expected, the first and last units of the row are best remembered, and therefore have, one may assume, less value in point of reproductive difficulty than those units situated nearer the middle of the row, which are more easily forgotten and therefore reproduced with more difficulty. Not only is this so, but in the case of rows near the threshold, where, in the attempt to preserve the unity of the row, the latter is sometimes repeated backwards, this method of valuation gives quite a wrong impression of the reproductive value of the first unit of the row, which would then be reproduced last in order. Most frequently its position is well known, its order in the row reproduced being due to the initial difficulty found in transferring the attention from the last given units, which tends to persevere to the detriment of those already presented[3]. This weakness has been recognised, and an attempt made to rectify it, in a method put forward by Wagner[1], the primary object of which is to obtain some definite relationship, based on the place values, between the units of the row, as given, and as reproduced. The following diagram[2] gives the most favourable case. Here, a series of eight rows containing from two to nine units is reproduced in the correct order:—

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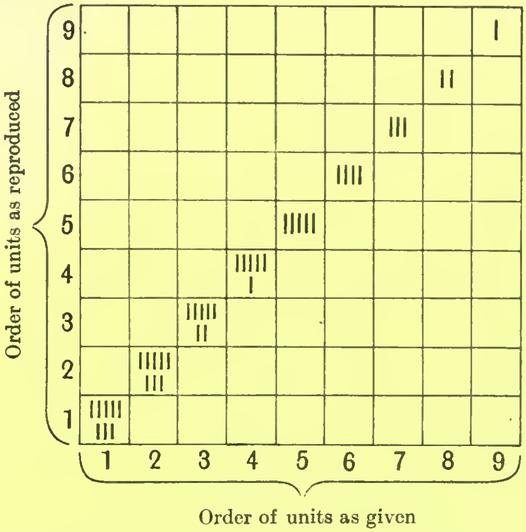
[1] Burt ("Experimental Tests of General Intelligence," This *Journal*, 1909), following Meumann (*Ztsch. f. exp. Päd.* I. 69) who, however, himself uses the negative method, adopts the following values:—For every unit correctly given and correctly placed, four marks; if one place out, three marks; if more than one, two marks. For a unit correctly placed, but with either the initial or final consonant or medial vowel wrong, three marks; if incorrectly placed, two marks. For those units rightly placed but with two components altered, two marks were given.

[2] Cf. Binet, "La mémoire des mots," *L'Ann. psychol.* 1894, 12; Bolton, "Growth of Memory in School Children," *Amer. J. of Psychol.* IV. 379.

[3] See note, p. 337.



The units, if all correctly reproduced, would lie as above in one diagonal —the 'chief diagonal'—of the diagram. Assuming, now, that the worst case is that in which all the units of the row are reproduced, but in their inverse order, then the diagram would be represented as on page 326.

In such a case as this, where eight rows successively ranging from two to nine units are reproduced in inverse order, there will always be four units in the chief diagonal. This number must moreover vary with the number of the rows. Hence, if m be taken as representing the number of units in the chief diagonal, when the rows are reproduced in inverse order (*i.e.*, in the worst case), and n the number of units in the longest row of the series presented, the following are the equations for uneven (m_1) and even (m_2) values of m:—

$$m_1 = \frac{n-1}{2}, \quad m_2 = \frac{n-2}{2}.$$

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[1] "Die rechnerische Behandlung d. Ergebnisse bei der Prüfung des unmittl. Behaltens," *Ztsch. f. exp. Päd.* 1913, 60.

[2] This diagram adapted from an already mentioned paper of mine ("Beziehungen zwischen d. Intelligenz u. dem Lernen u. Behalten," 43) was then used by me to illustrate differences in the reproductive ability of school children. These differences, however, for reasons then given, I did not attempt to evaluate more precisely.

Order of units as reproduced

9	|								
8	|	|							
7	|	|	|						
6	|	|	|	|					
5	|	|	|	|	|				
4	|	|	|	|	|	|			
3	|	|	|	|	|	|	|		
2	|	|	|	|	|	|	|	|	
1	|	|	|	|	|	|	|	|	|
	1	2	3	4	5	6	7	8	9

Order of units as given

If now r represent the number of units in the chief diagonal, s the total number of units presented and k any given reproduction of the rows, then

$$k = \frac{r - m}{s - m},$$

or, where n is an uneven number,

$$k_1 = \frac{2r - n + 1}{2s - n + 1},$$

and, where n is an even number,

$$k_2 = \frac{2r - n + 2}{2s - n + 2},$$

the values of k lying between 1 and 0.

This method, as the writer recognises, yields a very imperfect treatment of the results; it restricts the issue to too narrow a basis. It assumes, that while in the best case the rows are reproduced in the proper order, in the worst case they are reproduced in inverse order. In actual practice, the case assumed by Wagner as the worst is by no means actually the worst. Indeed, to reproduce a row of from six to nine units backwards is a feat requiring no small capacity. For a complete and regular backward reproduction of *long* rows, requiring as it does the breaking up of the forward associations of place and imagery, and the building up of new ones, calls for concentrated effort greater than that required for the usual form of reproduction. This evaluation, too, like those mentioned before, fails to take into full account the differences of place value in the units of the row. To repeat the medial members of a long row is a much more difficult task than to repeat the last, or, in some cases, the first. Nor can such a method make allowance for fluctuations in the accuracy with which the rows are reproduced, this varying from row to row and depending upon the nature of the row, and the behaviour of the learner. In some rows, once the first unit has been learnt, the remaining units come quickly and easily to the tongue; in others the effort is greater and the units come up in isolated fashion, the subject often having but a vague idea of their place.

III. THE SPAN OF PREHENSION.

Such then, in broad outline, are the methods hitherto adopted in the evaluation of the data obtained from experiments on immediate memory. As is evident, estimations of this nature are based almost entirely upon quantitative, rather than upon qualitative considerations. Moreover, in such cases, little attention has been paid to the analysis of the individual reproductions and to their varying differences. Table I, giving the number of times each row was correctly reproduced in the experiments described above, indicates this very clearly.

In the case of numbers, the threshold[1] may be taken as varying for the adults between the sixth and the seventh unit rows, for the intelligent children between the fifth and the sixth unit rows, and for the less intelligent between the third and the fifth. For syllables, the threshold is, of course, lower, ranging for adults from the fourth to the fifth unit rows, for the intelligent children from the third to the fourth, and for the less intelligent from the second to the third unit rows. In the syllable rows, the differences are more marked owing to the greater difficulty experienced in reading and reproducing syllabic forms. To what extent however such differences are dependent upon the greater difficulty found in the synthesis of the syllabic units of the row, to the child's difficulty in reading, or to its power of adaptation, cannot be here decided. The figures show, too, that the boys give

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[1] The threshold is determined by the longest complete row of units which can be immediately and correctly repeated.

TABLE I.

A. *Numbers (presented five times)*

No. of units in each row	Adults				Boys										Girls									
	Male		Female		Intelligent					Unintelligent					Intelligent					Unintelligent				
	X	*Y*	*O*	*P*	1	2	3	4	5	I	II	III	IV	V	*A*	*B*	*C*	*D*	*E*	*a*	*b*	*c*	*d*	*e*
3	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	4
4	5	5	5	5	5	5	5	5	5	5	5	5	3	1	5	5	5	5	5	4	5	5	5	2
5	5	5	5	5	5	4	5	4	4	4	5	4	—	—	4	3	5	3	2	4	2	3	3	—
6	5	4	4	4	4	2	2	2	—	—	4	—	—	—	2	—	2	2	—	—	1	—	—	—
7	3	3	3	3	2	—	1	—	—	—	—	—	—	—	1	—	—	—	—	—	—	—	—	—
8	1	—	2	4	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
9	1	—	—	—	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
Threshold	7	6–7	7	7–8	6	5–6	5–6	5–6	5	5	6	5	3–4	3	5–6	4–5	5–6	5–6	4–5	5	4–5	4–5	4–5	3

B. *Syllables (presented ten times).*

No. of units in each row	Adults				Boys										Girls									
	Male		Female		Intelligent					Unintelligent					Intelligent					Unintelligent				
	X	*Y*	*O*	*P*	1	2	3	4	5	I	II	III	IV	V	*A*	*B*	*C*	*D*	*E*	*a*	*b*	*c*	*d*	*e*
2	10	10	10	10	10	10	9	10	9	8	7	7	8	2	10	10	10	10	10	9	6	7	8	4
3	9	9	10	10	10	9	8	9	9	4	3	2	1	—	10	7	6	6	8	1	2	—	1	1
4	6	3	8	10	4	5	4	2	3	—	—	—	—	—	3	3	1	—	—	—	—	—	—	—
5	1	—	2	4	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
Threshold	4	3–4	4	4–5	3–4	3–4	3–4	3	3–4	2–3	2–3	2	2	—	3–4	3–4	3	3	3	2	2	2	2	2

better results than the girls. It is also interesting to note that the best results of the boys are not so very much behind those of the adults; indeed, probably, the span of prehension in a boy of the age of ten is not much below that of the untrained adult.

This table, however, gives but a very general idea of the differences revealed in the process of learning, as observed in different individuals, for the attempt to apprehend the row as a whole often persists to a marked extent in long rows which are distinctly beyond the threshold. Evidence of this is seen in the results shown in Table II.

Sections A and B of Table II show the results of the presentation of a series of syllable and number rows to all the subjects. In the fourth of the number rows, there is some uncertainty in the replies of the adults. The numbers 1, 3, 6, 7 follow the order in which these numbers are generally used, hence the forward associations linking 3, 6, 7 are stronger than the association between 6, 3, 7. In the case of the males, the order of the units in the sixth row shows more variation, the greater stress which has been placed upon certain of the units causing them to be reproduced first. In the seventh row this difference is still more marked and further some of the units here are forgotten. It is important, however, to notice that the row as a whole (*die Gesamtvorstellung*) is still remembered, though the constituent units may have lost their proper places. The repetition of the row maintains, too, in the main the freshness of the original impression. The same is to be noticed in the case of the boys and of the girls. Here again the fourth row has caused difficulties, not great enough, however, as far as the intelligent children are concerned, to destroy its unity. The fifth row, though some individual units are forgotten, exhibits similar features in the answers of 1, 2, 3, 4, and of *A* and *B*. In the sixth, the difference in the replies has become more marked. Here the influence of the span has disappeared in all but the answers of 1 and 3. One boy (1) has given the seventh row quite correctly, whereas the rest have only succeeded in reproducing scattered portions.

Owing to the more complex character of the units, the effort to reproduce the row as a whole is more clearly marked in the syllabic than in the number rows. The figures representing the threshold of immediate memory for syllable rows have been given above[1]. These figures, however, represent only the rows *correctly* repeated; they do not take into account those where the synthetic effort is still effectively

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[1] See Table I.

TABLE II A. *Numbers.*

Row as actually presented		Adults				Boys										Girls									
		Male		Female		Intelligent					Unintelligent					Intelligent					Unintelligent				
		X	*Y*	*O*	*P*	1	2	3	4	5	I	II	III	IV	V	*A*	*B*	*C*	*D*	*E*	*a*	*b*	*c*	*d*	*e*
I	3 8 2	+ + +	+ + +	+ + +	+ + +	+ + +	+ + +	+ + +	+ + +	+ + +	+ + +	+ + +	+ + +	+ + +	+ + +	+ + +	+ + +	+ + +	+ + +	+ + +	+ + +	+ + +	+ + +	+ + +	+ + +
II	6 1 3 5	+ + + +	+ + + +	+ + + +	+ + + +	+ + + +	+ + + +	+ + + +	+ + + +	+ + + +	+ + + +	+ + + +	+ + + +	+ + + +	6 2 3 5	+ + + +	+ + + +	+ + + +	+ + + +	+ + + +	+ + + +	+ + + +	+ + + +	+ + + +	6 3 1 5
III	3 6 8 1 4	+ + + + +	+ + + + +	+ + + + +	+ + + + +	+ + + + +	+ + + + +	+ + + + +	+ + + + +	+ + + + +	+ + + + +	+ + + + +	+ + + + +	6 3 — — —	6 8 2 5 1	+ + + + +	6 3 6 8 4	+ + + + +	+ + + + +	6 3 8 1 4	8 3 6 1 4	3 8 6 1 4	6 8 1 4 —	+ + + + +	3 8 1 4 —
IV	2 4 1 6 3 7	+ + + + + +	+ + + + + +	2 4 1 3 6 7	2 4 1 3 6 7	+ + + + + +	+ + + + + +	2 3 1 4 6 7	+ + + + + +	2 4 **1** 3 6 7	3 6 Pause 1 7 —	2 6 1 **4** 3 2 7	2 4 3 1 6 7	2 4 — — — —	4 5 6 7 2 —	2 4 6 3 1 7	2 1 3 4 5 6	+ + + + + +	+ + **+** + + +	2 **4** 6 1 — —	2 **1** 3 **6** 2 7	2 1 4 3 6 7	6 2 1 — — —	4 3 2 6 7 —	8 7 1 2 8 —

V	VI	VII
8 5 3 7 1 4 2	4 7 2 9 3 8 1 5	9 2 4 7 3 8 6 5 1
+ + + + + + +	4 7 2 5 3 8 1 9	9 **2** {8 7 3 5 6 1} —
+ + + + + + +	{4 7 2 **8**} {9 3 1 5}	9 2 4 3 7 8 — 5 1
+ + + + + + +	+ + + + + + + +	9 4 2 8 **3** — {6 5 1}
+ + + + + + +	+ + + + + + + +	9 2 7 6 {3 8 2 5 1}
+ + + + + + +	4 2 7 9 3 8 1 5	+ + + + + + + + +
{8 — 3 **7**} {2 1 4}	8 3 **1** 5 6 — — —	9 2 3 6 8 4 1 — —
{8 3 4 **7**} 3 1 2	4 2 7 **8** — 3 1 5	9 3 4 **7** 3 2 1 — —
8 — 3 7 1 4 2	4 3 7 8 1 5 — —	8 **3** 7 **9** **6** **1** **7** — —
8 7 1 {5? 4?} 3 2	8 9 5 **4** 7 3 2 —	8 6 7 5 3 4 — — —
{6 7 1} — {4 2} —	8 7 3 1 5 — — —	9 2 **4** 6 3 2 1 — —
8 — 3 **7** 4 1 2	9 3 **7** — 4 — — —	4 7 9 8 1 5 — — —
8 4 7 2 1 3 —	4 7 3 1 4 8 7 —	9 4 5 7 1 2 8 — —
5 — — — — — —	4 — — — — — — —	— — — — — — — — —
7 5 8 6 'and' 2 'and' 1 'and' 4	3 8 6 4 3 2 1 —	6 2 8 5 3 2 1 4 —
+ + + + + + +	9 5 3 6 4 1 — —	9 3 5 6 4 **3** {6 1} —
8 1 5 4 **3** {6 2}	1 9 4 3 7 8 — —	9 8 7 5 4 3 2 1 —
3 7 1 — — — —	4 7 1 3 8 9 — —	9 2 4 — — — — — —
8 — 3 7 — 4 2	**4** **2** **9** **8** **3** — {1 5}	**4** **3** **8** **5** 4 — — — —
8 **7** **4** **1** — — —	{2 **7**} {8 **5**} 1 — — —	**4** **7** **8** **2** **1** — — — —
8 1 3 7 2 — —	8 1 3 2 5 — — —	9 3 8 6 7 1 — — —
8 3 7 4 1 2 —	4 7 1 8 3 5 — —	{4 3 6 8} 1 — — — —
8 6 7 8 9 — —	2 3 — — — — — —	{9 8} 9 1 5 — — — —
8 5 2 **7** {6 3} —	9 4 7 **5** — {6 3} —	9 4 6 8 7 — — — —
8 5 6 2 — — —	4 8 2 1 5 — — —	4 8 5 1 8 4 — — —

(*a*) A pause in the reproduction is shown in heavy type.
(*b*) Units reproduced together are bracketed.
(*c*) A "+" signifies that the row is correctly reproduced. In cases of incorrect reproduction, the numbers are given in the order in which they were reproduced.

TABLE II B. *Syllables.*

Row as actually presented		Adults				Boys										Girls									
		Male		Female		Intelligent					Unintelligent					Intelligent					Unintelligent				
		X	*Y*	*O*	*P*	1	2	3	4	5	I	II	III	IV	V	*A*	*B*	*C*	*D*	*E*	*a*	*b*	*c*	*d*	*e*
I	kev	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
	ros	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2
II	gis	1	1	1	1	1	1	1	1	1	1	1	gis 1	3	kin 3	1	1	gib 1	ris 1	1	**1**	1	kiss 1	1	1
	reb	2	2	2	2	2	2	2	2	2	2	rev 2	riv 2	2	rib 2	2	2	rev 2	geb 2	2	**guv2**	2	rev 2	rev 2	rev 2
	suv	3	3	3	3	tuf *a* **then** 3 suf *b*	3	3	3	3	3	sub 3	3	1	1	3	3	3	3	3	sur 3	3	sev 3	sub 3	3
III	bok	1	1	1	1	1	1	1	hok 1	1	book1	1	hok 1	2 **Pause**	hen 4	bov 1	1	1	1	1	fog 1	kob 1	—	book 1	nog 1
	hiv	2	2	2	2	2	2	2	big 2	2	non 2	2	li' 2	—	sum 3	hib 2	2	3	2	2	zib 2	miv 2	—	him 2	niv 2
	mun	3	nem *a* mun *b* 3	3	3	num 3	3	num 3	dem 3	3	tuv 3	3	—	—	book 2	nen 3	3	4	num 3	—	van 3	dov 3	nook 2	3	nog 3
	deb	4	dub 4	4	4	4	dev 4	dev 4	nub 4	4	ded 4	def 4	—	1	1	dov 4	4	dev 2	sev 4	—	teb 4	4	dev 1	4	—
IV	pif	1	1	1	1	1	piv 1	1	—	—	1	1	1	—	—	pib 1	**3**	—	—	—	sif 1	pif 1	—	gif 1	nim 1
	hur	2	pur 4	2	2	2	hum 2	2	—	—	gun 2	huv 2	sam 2	—	sip 4	—	**2**	—	—	—	—	suf 2	—	nim 2	hif 2
	saf	nam 3	5 gaf 6	sap 3	3	**3**	sav 3	3	sav 1	**pim 1**	gif 3	sap 3	—	—	pad 3	2	—	—	1	sav 1	tos 4	guv 3	—	gif 3	sad 3
	nim	sif 4	num 3	4	4	4	rib 4	4	**2**	**suv 2**	—	niv 4	—	2	2	3	**1**	—	muv 2	dim 2	gim 3	nig 4	2	suf 4	gos 4
	gad	gaf 5	**2**	gav 5	5	saf 5	5	5	3	3	—	gav 5	—	1	1	pav 4	4	1	—	—	2	hed 5	1	—	—
V	mof	mok 1	hom 1	kof 4	gaf 4	mok 1	—	1	—	—	nuf 1	mok 1	**mun 1**	—	—	mob 3	—	—	—	—	gan 1	duk 1	—	din 1	nog 1
	heb	hif 2	—	—	vik 5	2	—	—	—	mun 5	nug 2	—	lik 2	—	—	—	—	**1**	—	—	vib 2	suv 2	—	gif 2	hif 2
	duk	3	huz 3	—	—	duf 3	gob 1	—	—	4	—	—	—	1	sik 4	—	—	2	—	1	sof 3	huid 3	—	sim 3	—
	gif	—	2	1	1	hip 4	hif 2	gib 2	**1**	guv 3	—	—	—	—	gan 3	—	gib 2	—	—	—	giv 4	fin 4	—	suf 4	—
	zan	van 4	zom 4	2	2	zam 5	zar 3	man 3	**2**	1	—	—	—	—	2	zeg 1	—	—	zib 1	2	—	—	2	—	—
	vir	dir 5	vor *a* **then** 5 vir *b*	3	3	6	giv 4	bez 4 zir 5	**3**	2	—	—	—	2	1	gan 2	1	—	2	ver 3	—	—	vil 1	—	—

(*a*) The numbers indicate the order in which the syllables were reproduced. In cases of incorrect reproduction, the syllables are given as they were repeated.

(*b*) See Table II A for explanation of brackets and heavy type.

present. Each adult has, for instance, repeated, or reproduced the fourth row completely, but not in the proper order, whilst in the fifth row *X*'s answer still bears marked traces of synthesis. A glance at the results obtained in the fourth and fifth rows by the intelligent boys and girls reveals the same characteristics.

These examples give evidence, then, that the span of prehension persists more or less actively in rows beyond the threshold, and moreover determines, in great part, the nature of the reproduction in such cases. A comparison of the difference of the threshold for numbers and for syllables strengthens the view that there is no very marked difference in the results obtained from intelligent and unintelligent children. What seems evident is that the apprehension of the row as a complex whole is determined largely by processes which vary with the degree of intelligence and with the mechanical or sensible nature of the material learnt.

Before, however, the varied effect of the span can be properly considered, a brief survey of the memory processes is necessary. Generally speaking, memory may be defined as the capacity to recall into the field of consciousness, after a given interval, impressions which have been once experienced, and to recognise these as old experiences. Thus, two processes have to be considered here, the learning of the material and its recall. The conditions attending the learning act allow of many variations. The material to be learnt can be presented in various ways and can be learnt in different forms, changing with the purpose which the learner has in view. Thus the material may be presented simultaneously or successively, it may be shown quickly or slowly, or again, it may be presented once or given several times. Subjectively considered, the act of learning varies with the purpose of the learner[1]; for instance, to learn what has to be recalled after a short interval of time is quite a different matter from learning that which has to be retained for a long period. It is the recognition of this difference that has given rise to the distinction between immediate and mediate memory. Immediate memory, in the strict sense of the term, is the capacity to repeat impressions which have not entirely disappeared from consciousness, the expression following immediately upon the impression. This repetition, *e.g.*, of a given row of syllables or numbers, depends in the first place upon the attention necessary for the building up of effective and durable associations, and in the second, upon what has been termed the 'span of prehension' or the "mind's power of *taking on* certain material[1]." By this is meant the ability to receive and combine into a complete whole (*die Gesamtvorstellung*) a given number of isolated impressions, and to repeat these accurately, immediately after the material has been presented. We are, therefore, concerned here with processes distinct from those involved in the remembering and reproduction of material after a lengthy period of time. In the latter case, it is the learning and retaining for a lengthened period that is important, whereas, in the former, it is the immediate repetition of material presented once only[2]. Strictly speaking then, immediate memory is distinguished by the *repetition* of impressions which have not entirely faded from consciousness and which, therefore, bear in their repetition distinct traces of their original freshness. It is clear that this form of memory[3] is found only in the repetition of those rows the length of which does not exceed the threshold, or, at any rate, exceeds it to a very small degree. Thus, when a subject repeats a row of six units correctly, but breaks down at a row of seven, the repetition of the six units is characteristically rapid, whilst there is often complete forgetfulness when a second attempt to repeat the units is made[4]. In such rows, an effort is made *to repeat the row as a whole*, though on again exposing the row, the subject is frequently unaware that individual members of the row were originally present[5]. In rows just beyond the threshold, one of two things may happen. Either the attention is concentrated on the whole row, when usually the span breaks down entirely and but a few units are reproduced in haphazard manner (see Table II A, rows VI and VII for numbers and Table II B, rows IV and V for syllables), or the subject attempts, often unconsciously, a division of the row. In the former case it may often happen that if the first units of the row are again exposed, the whole row, or most of it, is reproduced in regular sequence[1]. This fact would tend to strengthen the view that in immediate memory the attentive effort is directed to the synthesis of the elements into a combined whole in which each separate unit has almost entirely lost its individual character[2].

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[1] Cf. Meumann, "Beobachtungen über differenzierte Einstellung bei Gedächtnisversuchen," *Ztsch. f. exp. Päd.* 1912 456.

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[1] Jacobs, *op. cit.* 79.

[2] Cf. Meumann, *Ökonomie u. Technik des Gedächtnisses*, 41. Aal believes this can be more precisely formulated, see "Ein neues Gedächtnisgesetz," *Ztsch. f. Psychol.* LXVI. 41.

[3] So far as the learning of rows is concerned.

[4] As one child put it:—"You get them one minute, they go away the next."

[5] Cf. Nagel, "Untersuchungen über Grundfragen d. Assoziationslehre," *Arch. f. d. ges. Psychol.* XXIII. 211–12. Nagel, in testing his subjects by the scoring method on the knowledge of syllables contained in rows learnt a few minutes previously, found that many syllables forming the rows just learnt were, in the scoring test, not only not reproduced, but not even recognized. Nevertheless, when asked to repeat the row in which these apparently unknown syllables occurred, the subjects gave them without difficulty.

When longer rows are presented, the results and the processes involved are of a somewhat different nature. The row is now too long to allow of synthesis into a unified whole, since the first, and often the medial, units have disappeared from consciousness before the last have been presented. Hence the reproduction of such long rows has two stages. First those units are *repeated* which still ring and persevere in consciousness as primary images; then comes the *reproduction* of units, sometimes in pairs, and sometimes singly, as chance associations may occur. These two stages are quite distinct, and are usually distinguished by the quickness with which the elements in the first 'trip,' as it were, off the tongue, whilst, in the second, there is a conscious seeking for association and an effort to recall. Evidence of this change as the rows grow longer may be seen in the results given in Table II.

Table III, which deals with a series of syllable rows, shows similar features.

Taking the adult replies first, it is seen that up to the third row the influence of the span is still present. In the fourth case with the exception of *X* the synthetic effort has practically broken down, and the syllables are reproduced somewhat differently. In the replies of *Y* and *O* the syllables were reproduced slowly, usually with pauses between each, whilst in that of *P* an attempt was made to reconstruct the row in backward order but with little success. The fifth row was still further forgotten and practically no synthesis is observable. *O* and *P* repeated two or three of the late syllables quickly. Then came a pause, with the reproduction, after intervals, of the other syllables. In these long rows, *P* reproduced the first syllables by associating them with each other, or often with sensible words resembling them in sound, though this latter association was not a deliberate act during the presentation of the row. The same phenomena are to be noticed in the replies of the children. As the rows grow too long for their apprehension as a whole, so the results show the changed nature of the learning

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[1] See Table II B, row V. *X* had reproduced this row as there given. He felt, however, that the first syllable inhibited the reproduction of the others. I therefore gave him *mof*, when he at once gave me "*mof, heb, duk, van, dir.*"

[2] See Nagel, *loc. cit.* 209–213, on the influence of the *Gesamteindruck.*

TABLE III.

Row as actually presented		Adults				Boys					Girls				
		Male		Female		1	2	3	4	5	*A*	*B*	*C*	*D*	*E*
		X	*Y*	*O*	*P*										
I	sar	1	1	1	1	1	1	var 1	1	1	1	1	1	1	1
	tok	2	2	2	2	2	2	2	2	2	2	2	2	2	2
	vun	3	3	3	3	3	3	3	3	3	3	3	3	3	3
II	mal	1	1	mag 1	1	1	1	nog 1	1	1	nag 1	**val 1**	nal 1	1	tal 1
	teb	2	tem 2	teg 2	2	2	2	**tel 2**	teg 2	**2**	tog 2	**tub 2**	2	2	tib 2
	nog	3	mog 3	3	3	3	3	3	nob **then** nog 3	jog 3	sel 3	**nal 3**	3	nol 3	ma' 3
	jum	4	4	4	4	4	4	4	4	tum 4	mun 4	vem 4	4	jun 4	—
III	raf	1	1	maf 1	1	1	mal 1	ras 1	rab 1	1	mab 1	**mab 1**	rab 1	1	—
	mib	2	2	ris 2	2	2	—	—	mif 2	2	mis 2	3	—	jev 2	miv 2
	tes	3	tom 3	teb 3	3	3	jis 2	tem 2	**tev 3**	**3**	—	—	2	jom 3	tas 1
	kom	4	4 *a* | *b* kum | tom	4	4	**4**	kob 3	tom 3	4	kof 4	kon 3	—	rog 3	ral 4	3
	lev	5	5 lem | lev	res 5	5	meb 5	4	4	tes 5	num 5	—	**2**	ram 4	—	—
IV	jed	1	1	**1**	**tab 6**	1	—	1	jes 4	**1**	geg 1	—	leb 1	—	—
	num	2	—	—	**mon 5**	2	—	rom 2	—	mun 4	—	—	—	**4**	—
	lob	3	lab 2	**lum 2**	**leb 4**	**log 3**	—	rud 3	—	**kov 3**	lav 2	—	—	**gol 3**	—
	gav	4	gaf 3	—	**jab 3**	—	geb 1	—	1	—	—	—	—	—	ron 1
	fon	feb 5	gom 4	**3**	2	jeg 4	jon 2	—	pon 2	—	jog 3	**1**	—	**lon 1**	kon 2
	pag	num 6	fon 5	pav 4	1	—	fab 3	—	fag 3	**lag 2**	—	2	—	**gab 2**	3
V	dem	dev 1	jem 1	sun 4	**4**	jen 1	nem 1	den 1	—	jem 1	—	—	pev 1	—	—
	pav	—	nab 2	peg 5	—	—	jum 2	sav 2	—	kav 2	—	—	jum 2	—	—
	lut	jud 2	**3**	—	**5**	tuf 2	sam 3	**run 3**	**let 5**	—	—	—	—	—	—
	jen	luv 3	—	1	sug 6	—	nid 4	—	**jer 4**	—	—	—	—	—	—
	sug	4	—	—	1	—	jun 5	—	1	—	seg 1	—	—	sur 1	1
	rad	—	—	2	2	—	**nog 6**	—	nad 2	—	nab 2	**1**	—	—	nar 2
	nop	5	4 ?	3	3	—	lut 7	top 4	3	mop 3	—	2	—	—	3 ?

act. In the fourth and fifth rows the syllables are reproduced mainly as haphazard associations occur. If repetition takes place it is chiefly of the last two or three units, which are given quickly to make way for the more deliberative effort of reproduction. No attempt seems to be made, however, to repeat the whole or even grouped portions of the row, but merely to give isolated units as they are remembered.

Mere combined classifications of results, whether taken with regard to accuracy of form and of place, or of form alone, may thus often obscure factors which are of considerable importance[1]. Especially is this so with reference to evaluations based on the place value of the units. It frequently happens that in such long rows as V, VI, and VII of Tables II and III the last few units of the row are quickly and almost mechanically repeated, whereas the remainder are reproduced with considerable effort. These latter are recalled either singly or in association with others. If associated in pairs, and the row is not too long, they are correctly placed; if reproduced singly, only a vague idea of the place is present. A unit may thus be given as "near the end," "about the middle" and so on. In such cases, a vague awareness of the row as a whole remains. Often the first and last units are reproduced correctly, though not in their proper order. Nor, under these conditions, is it correct to conclude that the last units, because given first, are falsely placed, for though repeated first, their place in the row is generally perfectly well known *when* they are repeated. To reproduce them again in the hope of placing them properly, is to run the risk of building new associations of stress and of tone, which tend to weaken the original impressions. Especially is this the case with backward children. With them, the last elements of the row ring most strongly in consciousness, to the exclusion of most, if not all, of the other elements. Indeed, in material difficult of apprehension such as nonsense syllables, no synthesis is attempted; the child devotes its attention to the mere reading of the units. Hence the units remembered are reproduced singly, the last members of the row being given first, and in backward order. When questioned, however, the child is sometimes able to place them in their proper sequence.

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[1] See the replies of *Y* and *B* to the IVth syllable row in Table II B. *Y* gave the first and last syllables at once, then stopped and gave *num*, *pur*, adding that *pur* was second in the row. Then came *saf* and a little later *gaf*. In written answers the final result of this reply would probably read:—*pif*, *pur*, *saf*, *num*, *gad*, a result seemingly as good as those of *X*, *O*, 1, 2, or *A*. Similarly *B* reproduced the syllables singly with pauses between each reproduction. Answers of this nature are, however, of quite a different character from such replies as those of *X*, *O*, 1, 2, and *A*.

A comparison of the replies of the intelligent and backward children will serve as illustration. Sometimes only one or two units from a row of five are reproduced by an intelligent child, whilst his more backward companion will give two and will often attempt to reproduce a third and fourth. The two cases, however, are examples of different forms of learning. The backward child does not apparently combine the units; his attention is more often directed to each individual unit and, in reproducing, he is thus able to give two or three correctly. On the other hand, the intelligent child may find a row of five units just long enough for correct repetition. He, therefore, attempts to combine the units into a complete whole and to repeat them as such. At times he may succeed, at others he breaks down entirely. In the longer rows, however, the first few units are held, as it were, in suspense whilst the last are being presented. As soon as the whole row is exposed, these last units are immediately repeated as a group, whilst the first units of the row are reproduced, as chance associations occur. In these rows, therefore, there is no longer one complete effort. Some units are repeated quickly, others reproduced more slowly, often by means of association with sensible words. The rows of syllables in Table IV may be taken as examples.

Here the third row reveals the influence of the span in each of the adult replies. The fourth on the other hand shows different characteristics. In the cases of *X*, *Y* and *P* efforts were made at a repetition of the whole row with but doubtful success. *O* found the third row just a little too difficult for her and so, in the fourth, endeavoured to give as many syllables as possible. The first to the fourth units were repeated quickly, as a group; then, after a pause, came the fifth and sixth, *fob* being probably remembered through association with fob (a watch pocket), and thus reviving the association with *kir*, the unit next following. In the fifth row, the span has practically disappeared. *X* reproduced the syllables in pairs, with a pause between each, in complete contradistinction to the rapid form of the repetition which he adopted in the shorter rows. *Y* adopted the same procedure, *jan*, a syllable which he had incorrectly reproduced in the fourth row, being here correctly given instead of *raz*, a syllable of like sound, occupying the same position in the row. The case of *P* is of interest. Her threshold fluctuated between rows of four and five units, the failures in rows of five syllables being mainly due to transposition of the consonants.

TABLE IV.

Row as actually presented		Adults				Boys					Girls				
		Male		Female											
		X	*Y*	*O*	*P*	1	2	3	4	5	*A*	*B*	*C*	*D*	*E*
I	kuv	1	1	1	1	1	tuv 1	1	1	1	1	1	1	—	1
	las	2	2	2	2	2	2	2	2	2	2	2	2	taj 1	2
	tej	3	3	3	3	3	3	3	3	3	3	3	3	2	3
II	jad	1	1	1	1	jab 1	jup 1	1	—	1	jav 1	—	**jav 1**	dad 1	—
	lup	2	**2**	2	2	nup 2	2	2	lip 1	2	2	**las 1**	**lik 2**	—	1
	nig	3	3 *a* piz 4 *b* nig	3	3	liz 3	lik 3	3	nub 2	pig 3 (corrected)	niv 3	3	tes 3	—	nez 2
	pez	4	jez jez	4	4	tej 4	4	4	jez 3	jez 4	tes 4	**teb 2**	—	puz 2	ped 3
III	gur	1	hur 1	1	1	1	1	guv 1	—	1	—	—	—	—	—
	haf	2	gav 2	2	2	2	hub 2	hir 2	—	2	—	—	—	—	—
	sib	jib 3	hir 3	3	3	**sir 3**	sid 3	—	1	vid 3	seb 1	**sub 1**	—	sev 2	1
	jor	sor 4	4	4	4	**nov 4**	job 4	—	2	sur 4	vor 2	**2**	—	zor 1	jar 2
	vad	jev 5	5	5	5	sar 5	had 5	—	3	—	tis 3	3	1	—	vor 3
IV	kir	*a* jir **(then)** *b* kir 1	1	5	1	kim 1	—	—	—	kiv 2	—	**2**	—	—	ker 1
	fub	—	—	fob 6	bir 2	—	—	—	—	nur 3	- -	—	—	—	—
	jan	2	jiv 2	1	jak 3	—	heb 1	bav 1	—	kel 4	3	—	1	—	2
	tek	3	hir 3	2	nek 4	—	jib 2	nek 2	**1**	—	**jeb 1**	—	—	—	—
	biv	—	—	3	tiv 5	—	tav 3	3	2	—	—	**vib 1**	—	kiv 1	—
	nol	—	4	4	—	—	4	—	3	1	**vor 2**	3	2	2	3
V	mip	—	vip 1	—	nap 4	hip 1	—	—	—	—	zip 2	—	—	1	nip 1
	vot	—	—	—	vop 5	—	—	pim 1	—	vol 3	—	—	—	—	—
	raz	2	jan 3	1	—	—	—	pep 2	—	—	—	**4**	—	baz 2	—
	fum	**1**	—	2	—	—	rub 1	**ves 3**	—	kut 4	—	**hev 5**	—	3	—
	pep	3	mep 4	3	1	—	vib 2	—	**1**	—	3	**pip 3**	**1**	—	—
	vin	4	—	4	2	—	taz 3	pev 4	**2**	1	—	1	**hin 2**	—	—
	hes	—	2	5	3	—	veb 4	vin 5	ves 3	2	1	2	ves 3	—	2

The unity of the row was, however, preserved in each case. In the rows of four units, the procedure varied. Sometimes an attempt was made to repeat the row as a whole, with results similar to those given here. At other times, reproduction took place on the lines shown in the fifth row, *i.e.*, the last few syllables were repeated quickly, and then effort was made to reproduce the rest of the row.

The same is to be noticed, though not to such a marked extent, in the replies of the intelligent children. The answers to the second and third rows vary considerably as the synthetic effort succeeds or fails. In the fourth and fifth rows there is some improvement, owing to the different aim kept in view, for in these rows the children seem to aim at reproducing as many syllables as possible. With no introspection to guide us, however, it would be dangerous to base any comparison on such purely objective results.

IV. ERRORS OF PARTIAL REPRODUCTION.

Further effects of the differences in reproduction noticed above are to be observed in an examination of the numbers and syllables incorrectly reproduced. In the short rows, where the primary sensory elements are still 'ringing' in consciousness, characteristic interchanges of consonants and vowels take place. It is rare, however, that false syllables or sensible words are inserted. The row is a short complete unit in consciousness, and any change in its original constitution follows along lines which vary according to the nature of the units and of the individual. The first evidence of the weakening of the original impressions is in an interchange of the units of the row and, in the case of syllables and words, the interchange of consonants and vowels. The following reproductions are examples of this:—

A. NUMBERS.

Row as given	Row as reproduced			
5	5	5	5	5
7	9	2	2	7
2	2	7	7	9
9	7	9	9	2
6	6	6	6	6

B. Syllables.

Row as given	Adults				Boys				
	X	*Y*	*O*	*P*	1	2	3	4	5
vig	1	1	zig 1	1	1	vin 1	1	nig 1	1
nup	2	nuf 2	2	2	2	nub 2	nep 2	vup 2	2
tem	3	teb 3	3	3	3	sen 3	tum 3	3	ten 3
sif	4	sig 4	4	siv *or* sif 4	4	fis 4	4	4	4

The incorrect sequence characteristic of numeral rows, and the transposition of consonants or vowels in syllables and words otherwise correctly given, is probably the first stage of forgetting. It takes place, generally, in rows near the threshold where the conception of the units of the row, combined to form a complex whole, is still present in consciousness, but where however the individual members are becoming less firmly fixed. In slightly longer rows appears the further stage, when the units are misplaced or sometimes entirely forgotten. In such cases, the unity of the row is broken, though the conception of the whole row as a scheme (*die Gesamtvorstellung*) still persists. It is as if one were able to pronounce a word but not to spell it. The following tables illustrate this:—

A. Numbers.

Row as given	Reproductions			
9	5	9	5	—
3	—	3	3	—
5	8	—	—	5
8	9	8	8	8
1	1	1	1	1
6	6	6	6	9

B. Syllables.

Row as given	Adults				Boys				
	X	*Y*	*O*	*P*	1	2	3	4	5
vem	1	vom 1	rem 1	1	1	rem 1	1	**vub** 1	1
rud	nub 2	—	rub 2	rub 2	rup 2	nud 2	faz 2	rup 4	2
sep	3	—	sek 3	sef 3	sem 3	3	sup 3	mez 3	—
faz	viz 4	—	fab 4	—	ras 4	man 4	vem 4	—	—
mig		—	riz 5	4	vig 5	giv 5	5	**big** 2	—

The numbers need no comment. In the syllable rows *X* was disturbed by the sibilants and gave *viz* a combination of *faz* and *mig* as the last syllable. *Y* was not sure of the first syllable, stopped to consider it, hesitated and lost the row. *O* gave the complete row, with its units, however, much weakened. *P* forgot the fourth unit, the transition from the *p* in *sep* to the *f* in *faz* tending to a confusion of the two syllables. The replies of the boys were similar. 1, 2 and 3 gave the whole row in varying degrees of accuracy. 3 forgot the second syllable and combined it with the third to give *sup, faz* being transferred from the fourth to the second position, in consequence, probably, of the tendency to accentuate the penultimate syllable in rows just too long for accurate repetition[1]. In the cases of 4 and 5 the reproduction of the row is not so correct. 4 gave the first and last syllables with pauses between each, while *rup* and *mez*, combinations of the second and third, and of the fourth and fifth respectively, were (after a pause) given together.

In the longer rows, the nature of the errors becomes naturally more complex. In the case of syllables, associations with sensible words often lead to confusion. Moreover, associations with other syllables having similar consonants or vowel sounds continually occur. This, too, is the cause of the incorrect reproduction of words and syllables of similar sounds occupying similar places in rows previously exposed[2].

The frequency of the mistakes in reproduction has given rise to characteristic differences in the work of intelligent and backward children. One of these is the perseveration of distinct types of combinations. The mechanical act of learning is dependent, as Müller[3] points out, on two factors. These are, first, association, and, secondly, the physiological tendency of impressions to persevere for some time with somewhat of their original freshness. In everyday life this phenomenon plays a useful *rôle*. When, however, it gets the upper hand, the act of learning suffers considerably. The undue perseveration of mental images has often been noted as a sign of backwardness. In the learning of rows of numbers, syllables, or sensible material it may be noted in two forms[4]. In the first, one unit or combination of units may dominate the repetition of a row to the disadvantage of the others; in the second, certain combinations appear row after row in almost regular sequence. It is probable that the constant persistence of one image and its reproduction is due, in great part, to lack of ability to concentrate upon the other units of the row. The persisting image is constantly forced into the foreground, and the child is neither able to repress it, nor to concentrate upon the reproduction of the remaining units. Intelligent children attempt to give a complete reproduction of the row, or, if this is impossible, to reproduce associated units. With backward children, the persistence of the same image breaks up the associations and so spoils the reproduction of the row.

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[1] See p. 347.

[2] See the reply of *Y* to Row V in Table IV.

[3] Müller u. Pilzecker, *Exper. Beiträge zur Lehre vom Gedächtnis*, 75.

[4] Cf. Rusk, "Mental Association in School Children," This *Journal*, 1910. Rusk distinguishes between Perseveration and Persistence—"the repeated re-instatement in consciousness of the same image." This "perseverance of content in imagery," he terms persistence to distinguish it from "the mere perseverance of form." Cf. Jesinghaus, "Zur psychologischen Theorie des Gedächtnisses," *Psychol. Stud.* VII. 365 ff.

The first of the above-mentioned modes of perseveration, that in which certain elements tend to dominate reproduction within the row, is not to be confounded with the mere interchange of vowels or consonants. In the former case, a consonant or vowel, or even a whole syllable, may interfere with the reproduction of the row. Especially is this so when a syllable or word happens to present any special difficulty in reading. Thus, for the row *het*, *gos*, *lun*, *vap* was given *hes*, *gos*, *lun*, *ves*, the combination of *het* and *gos* having been more strongly impressed to the disadvantage of *het* and *vap*. Similarly, the row *per*, *hik*, *vud*, *kes*, *nem* was reproduced as *ruk*, *tus*, *nus*, *vut*, *tiv*; whilst the row *mof*, *heb*, *duk*, *gif*, *zan*, *vir* gave *din*, *gif*, *sim*, *suf*[1]. In number rows, numbers following the numerical sequence are often inserted (*e.g.*, see Table II A), whilst often even numbers are followed by even and odd by odd.

The tendency of certain elements to dominate reproduction is, perhaps, not so characteristic a form as that of iteration[2]. This, too, is a mode of perseveration found most frequently in the reproductions of backward children[3]. It occurs, not only in senseless syllables, but also in sensible words, in the latter case showing, as Burt points out[4], that sensible words are often not associated with any meanings, but are reproduced as primary sensory images. When numbers are exposed, a combination of two or three digits, once associated, is reproduced time after time. In many cases, the reproduction takes the form of mere repetition of the digits in numerical sequence. Thus the row

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[1] See Table II B.

[2] Wreschner, "Die Reproduktion u. Assoziation von Vorstellungen," *Ztsch. f. Psychol.* Ergänz.-Bd. III. 237 f. Chap. on *Perseverationstendenzen*.

[3] Meumann, "Intelligenzprüfungen an Kindern d. Volkschule," *Ztsch. f. exp. Päd.* I. 67, 96.

[4] *Loc. cit.* 144.

8 2 9 4 6 5 gave 8 9 (pause) 6 4 5. The succeeding rows of the same series were then reproduced as follows:—893745; 893472; 89362; 8 9 7 3 2 5, the figures 8 9 3 appearing in each reproduction. In another series 8 1 3 7 was given in almost regular sequence. Combinations of this nature do not appear to persevere in more than one series, but seem to be built up anew in the initial short rows of each series and then to be forgotten as soon as the series is ended. These short rows are often repeated correctly, though their dominant units, or the combinations composed of them, will often persist and disturb the reproduction of the longer rows of the same series.

Syllabic rows show this persistence in still more exaggerated form. Indeed, the strangeness of the material and the resulting difficulty in reading may account for the fact that a form once rendered familiar is not only reproduced, but even read time after time. In many cases, sensible words similar in sound to a unit of the given row are substituted for this unit, and then tend to persevere. The following answers to a series of seven syllabic rows (see Table IV), consisting respectively of from two to eight syllables, will serve as examples:—

Rows of	2	3	4	5	6	7	8	syllables
	vas ged — —	lass gev jess —	lak giv ged —	gab giv tos giv	kav giv tos —	van gav gos vip	kal giv gas —	

The first row of two units (*vas*, *ged*) was read correctly. They have, however, almost completely dominated the reproduction of the following rows of the series. Often, as is the case with *tos* in the above example, units appear at intervals and are then forgotten.

Another factor influencing correct reproduction is that known as regressive, retroactive, or backward inhibition[1]. The units composing rows successively presented do not disappear simultaneously from consciousness, but fade away in more or less regular sequence. The recall of any member of a given row is therefore dependent, in a large degree, upon its place in the row, the nearer the end of the row the fresher is its primary imagery. Thus, the last units presented exercise an inhibitory influence on their more remote fellows, so as often to cause them to fade away entirely, the inhibition becoming more effective the further removed the units are from the end of the row. One reason for this is that the persistence of the primary imagery of the units exposed last weakens those previously presented, so that the latter are often forgotten. This occurs chiefly in the longer rows, though it is often met with, in the case of backward children, in the shorter ones. The child being unable to overcome the distraction caused by the persisting images of the units last exposed[1], these are repeated more or less correctly and the first units are then given after frequent pauses. It may, therefore, often happen that the backward repetition of a row is a device resorted to in order to obtain some measure of success. Those units exposed last have first to be rapidly repeated, before attention can be concentrated on the rest of the row.

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[1] Cf. Müller u. Pilzecker, *op. cit.* 144–178. They distinguish between effectual generative and retroactive inhibition.

Regressive inhibition is not an essential sign of backwardness; intelligent children often have recourse to backward reproduction as the best and easiest, if not the only method. When, however, it is found persisting in row after row it may be taken as symptomatic of low intelligence. The inhibition may manifest itself in various forms, according to the rate at which the row is forgotten. The whole row, for example, may be repeated in inverse order, portions of it may be combined and reproduced as a group, or, where the row is long, isolated units may appear in haphazard order. The backward reproduction of a whole row is not of frequent occurrence, though often unintelligent children reproduce even rows of two units backwards. The following are the reproductions of four children (4 an intelligent boy and *B* an intelligent girl; V a backward boy and *c* a backward girl) when given rows of from two to five syllables:—

I.

Row as given	4	*B*	V	*c*
taj	1	1	2	1
kos	2	2	1	2

II.

Row as given	4	*B*	V	*c*
gub	1	1	3	gef 1
tef	2	2	2	tuv 2
sur	3	3	1	sef 3

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[1] Cf. Galton, "Notes on Prehension in Idiots," *Mind*, 1887, 82.

III.

Row as given	4	*B*	V	*c*
ser	kur 1	1	tav 4	—
kav	sav 2	2	vuz 3	—
luf	3	3	luv 2	lef 2
tig	4	4	1	1

IV.

Row as given	4	*B*	V	*c*
per	ker 4	ver 1	—	—
hik		—	—	—
vud	1	—	pim 3	—
kes	2	2	2	2
nem	3	3	1	nen 1

Up to the third row, the faults of 4 and *B*, the intelligent children, are not serious. 4, in the third row, however, has transposed the initial consonants in the first two syllables. Both results show that a complete repetition of each of the rows I to III has been attempted. In the fourth row, 4 repeated the last three syllables in their proper order, then paused in an endeavour to get the first two, and finally gave a combination of them in *ker*. In the case of *B*, the failure to reproduce is more marked. Every syllable was reproduced slowly, with distinct pauses between each reproduction, and it is clear that but a vague conception of the whole row was present. Quite different is the nature of the reproductions of V and *c*, the two backward children. V gave the first two rows backwards; asked to repeat the second in proper order, he failed completely. In the third and fourth rows, the last two syllables were given slowly and in inverse order; then after a pause came vague guesses at the other syllables. The case of the girl *c* is similar. After the second row, consisting of three syllables, this child invariably gave the last two syllables of the row, sometimes correctly, often not, and always in their inverse order.

V. IMPRESSION AND EXPRESSION[1].

Finally, the manner in which the material is learnt and reproduced by intelligent and by backward children gives typical differences. When an intelligent child is faced with a row, the units of which are successively presented, it reads each unit deliberately and clearly, with no marked emphasis until the end of the row is reached. The last unit is, however, read with a distinct accent, implying that the end of the row has been reached, and that, moreover, each unit has been read, not as an isolated element, but as a connected member of a definite whole. In rows near the threshold, this method of reading may often give unfavourable results. The child tends to be uncertain of the length of the row and, having lost count, is not sure which is the last unit. In such cases, the end accent often falls, not on the final unit but on the penultimate (the last being read alone and often with a note of unexpectedness). When such is the case, the reproduction suffers. The last two units, strengthened by this involuntary place accentuation, are immediately repeated, but the unity of the row is broken and only scattered units of the remainder are reproduced. This factor probably accounts for that state of confused forgetfulness observed in the attempt to repeat rows just too long for correct repetition. When such rows are repeated correctly, they are given rapidly, often more rapidly than they were presented. As one subject put it, "the numbers come naturally," the same posture, tone and accent being adopted in both cases.

Backward children, on the other hand, rarely seem to attempt any synthesis of the rows. The latter are read in an even and monotonous tone, the units being frequently shouted aloud, with a heavy pause between each. Further, every single act of impression seems to constitute a single isolated event. When the units are exposed by means of an apparatus[2], the backward child is often found reading the syllable, or word, as the next is about to be exposed. The reproduction follows in the same isolated and mechanical manner, a pause occurring between each unit, in complete distinction from the rapid repetition of the intelligent children. The pause between the impression of the row and its reproduction is also longer in the case of backward children. They seem to find the transition from the act of impression to that of reproduction a task of some difficulty. It is this facile adjustment to the varying aim in learning which seems often to play a large part in the determination of the results[1].

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[1] See my "Beziehungen zwischen d. Intelligenz, etc.," 61-2.

[2] It is not without interest to note that the exposure of the rows by means of the cardboard sheet seems to present difficulty to those who learn mainly by means of visual imagery. The moving of the sheet breaks up the continuity of the imagery and thus of the row.

VI. CONCLUSIONS.

I. Inferences previously drawn from experiments on immediate memory take no account of individual differences in the processes of repetition and of reproduction, of repetition in rows near the threshold and of both repetition and reproduction in those rows too long to permit of apprehension as a single whole.

II. In the learning of rows near the threshold, intelligent children apprehend the row as a complex whole, each unit being linked with the others and regarded as forming part of the whole. Backward children tend to learn each unit as isolated and almost as self-complete.

III. Perseveration in the reproduction of the rows may take place in two forms:—

(*a*) in the undue perseveration of a dominant number or consonant or vowel sound in the reproduction of a single row, or,

(*b*) in the appearance of combinations of units, or of single units, previously given and perseverating throughout a series of rows.

IV. The present experiments confirm previous work in showing that the influence of perseveration and of regressive inhibition, whilst not in themselves indicative of backwardness, are, however, more marked in the case of backward than of intelligent children.

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[1] See Burt, *op. cit.* 168.

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SOME OBSERVATIONS ON PSYCHOLOGICAL CONTRAST.

BY J. C. FLÜGEL AND WM. McDOUGALL.

I. *Introduction. Example of 'psychological contrast'—Inadequate 'set' as a condition of contrast—Relation of contrast to 'absolute impression' and the process of comparison—Successive and simultaneous contrast not essentially different—Aim and nature of present experiments.*

II. *Investigations into the occurrence of psychological contrast effects on—*
  - (i) *Judgment of lifted weights.*
  - (ii) *Recognition of colours.*
  - (iii) *Judgment of length of lines.*
  - (iv) *Judgment of width of angles; no contrast manifested.*
  - (v) *Judgment of duration.*
  - (vi) *Judgment of rate of seen motion.*

III. *Conclusion.*

I. INTRODUCTION.

THE present paper contains an account of some experiments made with the object of investigating the influence of Contrast as manifested in various mental operations. Having regard to the comparatively small amount of experimental work hitherto done in this field (if we except that on Physiological Colour Contrast) we aimed chiefly at finding out under what conditions Contrast does or does not occur, and at devising experiments likely to yield interesting results in future researches, rather than at making any elaborate investigation or measurement of contrast effects under any one set of conditions. We are aware that, owing to our having adhered to this point of view throughout our experimentation as well as to the rather short time we had at our disposal, the number of observations recorded under each set of conditions with which we experimented is, in many cases, much smaller than could be desired. We feel, however, that some of our results are sufficiently interesting to justify their publication, both as supplementing the too scanty work already published on this subject, and as suggesting lines along which further investigation might profitably be made.

The facts of Contrast have on the whole received but scant attention from experimental psychology. Since the unsuccessful attempt of Helmholtz to account for all Colour Contrast as supersensory in nature, attention has for the most part been concentrated on cases of Contrast which seem to be due primarily to physiological conditions in the sense-apparatus concerned; and, while it has been generally admitted that there exist contrast effects which cannot be thus explained, little effort has as yet been made to consider more closely the true nature of these effects or to investigate them experimentally.

The term 'Physiological Contrast' has been used to designate all contrast effects which seem to be determined by reciprocal modification of sensory processes, whether in the sense-organ or at some higher, yet infra-conscious, level of the sense-apparatus. On the other hand a large number of contrast effects seem to arise through distortion of judgment, and these are conveniently distinguished by the term 'Psychological Contrast.' It is to instances of the latter kind that our present observations are confined.

It is at once obvious that Psychological Contrast, so understood, may be involved in many different mental operations and in processes of very various degrees of complexity. This being the case, it will perhaps be well to begin by citing some experiences of ordinary life, which, we think, may fairly be regarded as examples of such Contrast.

A classical example is the effect produced on the appearance of a man of medium stature by the presence of another man unusually tall, or short. The contrast effect produced under these circumstances is, indeed, often very striking. Persons of ordinary size will suddenly appear to become puny and insignificant if a giant enters the room; while the same persons in the presence of a dwarf are apt to impress us as exceptionally tall and well developed specimens of humanity.

Contrast of a different kind (to which Wirth[1] has called attention) has been experienced by everyone on returning home after a considerable time spent in some other place. Many of the details of the house with which we are familiar appear to have been altered during our absence; thus the rooms may appear to be smaller or lower, the steps higher or narrower, the chairs higher, the door handles lower than they were before. When we seek the cause of these apparent changes, it will be found usually to be of the nature of a contrast effect induced by the character of our surroundings during our absence from home. If the rooms appear smaller, it is because the rooms in which we have lived while away were larger than those at home; if the chairs appear lower, it is because those which we have of late been using were higher.

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[1] "Vorstellungs- und Gefühlscontrast," *Ztsch. f. Psychol.* 1898, XVIII. p. 49.

Another contrast effect sometimes experienced on returning home is one which affects the apparent physical characteristics of the country. Thus to an Englishman just returned from Switzerland the hills of his own neighbourhood may appear to have shrunk very considerably and their slopes to have become less steep than formerly; while after a visit to Holland the same hills may appear to have greatly increased in height and steepness.

Again, if in the country in which one has been staying the people are of exceptionally large or small stature, the apparent size of a fellow countryman may be affected in much the same way as if a much larger or smaller person were actually seen alongside him. Other characteristics than size may be similarly affected. An interesting example of this experienced by one of us was produced by the oblique position of the eyes in the Chinese countenance; after living in eastern Asia for upwards of a year, the eyes of Europeans seemed to him to slope outwards and downwards, an appearance which repeatedly forced itself on his attention.

A contrast effect familiar to cyclists and motorists is often produced on changing from one gear to another. After running for a considerable time on a high gear, our progress often appears to have become very slow when we are compelled to put in a low gear; while, after mounting a long hill on the low gear, when the top gear is put in again we seem to dash off at a greater speed than we had before attained under similar conditions.

Marked contrast effects are often brought about by sudden atmospheric changes. A storm coming on suddenly at mid-day will make the landscape seem darker than it would do under the same degree of illumination towards evening.

A very striking but complex case of contrast has been observed by one of the present writers while travelling. On leaving England and entering a country with which he was less familiar, but which he had nevertheless visited several times, a distinct feeling of the comparative strangeness and foreignness of things about him was experienced. A short time afterwards on returning to this same country after a visit to another country with whose language and customs he was almost totally unfamiliar, he experienced the opposite feeling—that of being 'at home' in an environment to which he was accustomed.

Together with the cognitive element of Contrast there is often to be found an affective element, which may indeed on occasion become the predominant feature of the total mental state. Thus, to take a very simple case, a cool wind may seem delicious when we are overheated, while a warm fire is most appreciated when we are cold. On a higher level, it has been often remarked that joy is the more profound when it follows after sorrow, while sorrow is intensified by the thought of our own past joy or the present joy of others[1].

Contrast is similarly operative as regards our moral and aesthetic judgments. An example of this latter class may be cited from the experience of one of us. Sitting in the reading room of an hotel, he had been occupied for about half an hour in looking through some volumes of a German comic periodical. Tired with gazing at the rather gross caricatures which form the prominent feature of this publication, he, at length, closed the book before him and turned his attention to the other occupants of the room. To his surprise, the men and women, who, but a short time before, had appeared to possess no source of particular attraction, now seemed suddenly to be endowed with quite exceptional beauty both of face and figure—a beauty which was not merely physical but which appeared to indicate the presence of the highest moral and intellectual characteristics.

These examples may perhaps suffice to remind us of the great range and variety of the phenomena of Psychological Contrast and enable us to give a provisional description of their nature.

If we seek a definition of Psychological Contrast that will be applicable to all the above examples, we may perhaps describe it as *an effect on our judgment of some quality of an object, produced by the experience of other objects, which, though of the same kind, differ from it in respect of the quality in question; the effect being of the nature of an apparent magnification of this difference.*

Contrast, then, is brought about in and through a judgment; and we may not unreasonably expect some light to be thrown on the nature of Contrast by a brief consideration of the conditions of judgment, and conversely. Without entering into the numerous detailed descriptions of judgment which have been given by psychologists and logicians, we may, we think, take it as generally accepted that judgment is of the nature of an assimilative process in which new impressions are received and interpreted in the light of past experiences. We can only judge an object as 'such-and-such' when we have already had experience of similar objects; and our actual judgments concerning any new object will depend upon its relation to similar objects that we have known in the past. Thus, in the main, we judge an object 'large' or 'small' according as it is larger or smaller than the majority of its class with which we have already met. The new object is not, of course, in most cases consciously compared with the similar objects we have seen before. What usually happens is that these objects have set up in our minds a standard, in the light of which we judge all newcomers of the same class; though here again there is generally no conscious comparison; all that occurs in consciousness is that the new object is immediately experienced as *absolutely* large or small. It is only when we seek to give a reason for our judgment that its ground is seen to lie in a difference of the new object from those of the same kind we have seen in the past.

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[1] Cf. A. F. Shand, *The Foundations of Character*, 334 ff.

The studies of the motor 'set' or disposition (*motorische Einstellung*) made by Prof. G. E. Müller and his coadjutors[1] afford valuable guidance when we consider this problem of contrast. It was shown by them that the repeated lifting in immediate succession of a pair of very unequal weights (light and heavy) results in a motor set which reveals itself as a tendency for the strength of the motor impulses of any two successive lifting-movements to be adjusted respectively to light and heavy weights, *i.e.* while the subject intends to lift with equal energy, the outflowing motor impulses are so modified by the motor set (which presumably has a subcortical seat) that the first efferent impulse is relatively weakened and the second relatively strengthened.

The same group of workers has shown that in comparing successively lifted weights (or other objects in respect of their sensible qualities), our judgment is frequently determined by an 'absolute impression' of heaviness or lightness (of brightness, of loudness, etc.), which accompanies or qualifies the perception of one or other of the two objects compared. Such absolute impression is apt to qualify the perception of any object of a kind with which we are familiar, but which slightly differs in respect of some one sensible quality from the objects of that kind which we are accustomed to perceive—as when we lift a water-jug which we are accustomed to find full of water, but which on this occasion is unexpectedly empty, we receive an 'absolute impression' of lightness or lack of weight. Now, in this last case it seems clear that the absolute impression is doubly conditioned; on the one hand, by a motor set which has been formed through repeated liftings of the object on previous occasions, and which determines us to put forth a motor impulse adjusted to the usual weight of the object; on the other hand, by a perceptual or cognitive set formed by previous experiences of the normal effects on the object of the effort put forth; this perceptual set assimilates the sense-impression if the effect of the effort is the normal or ordinary effect; but, if the object is unusually light (the jug empty), the effect of the effort is other than the usual one, the object flies up rapidly, and the perceptual set, being adjusted to assimilate an impression of slower movement, is not adjusted for assimilation of this impression of more rapid movement. This lack of exact adjustment of the perceptual set seems to be the condition of the rise of the absolute impression by which our judgment in such a case is determined.

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[1] *E.g.* Laura Steffens, "Über die motorische Einstellung," *Ztsch. f. Psychol.* 1900, XXII. 241.

We bring forward these considerations in this place because it seems to us that for the explanation of many, perhaps all, instances of Psychological Contrast we have to assume the operation of such motor or perceptual sets. Whether any line can be drawn between the motor and the perceptual sets seems to us a difficult question; but it seems clear that they are at least closely analogous in nature and mode of operation, and perhaps essentially of the same nature; for the motor adjustment through which a motor set is formed is always part of the total perceptual reaction; and we may henceforth speak of the set without insisting on the distinction of motor from perceptual sets.

The application of this notion of a set to the explanation of many contrast effects is obvious enough.

Contrast effects are undoubtedly most striking when they affect the impression made on us by an object with which we are already familiar; in which case we are able to compare the present impression with that made previously by the same object in the absence of the contrast effect. Of this nature were most of the examples given above. Take the case of the door handles of our home which seemed to have become lower after some time spent in another house. In this house the door handles were higher than at home, and while living in it we had so modified the set by means of which our hands were guided to the handles that, after a short time, we adapted ourselves to the new conditions by always moving our hands to a higher level than before. When we return home this modification of the original set persists; we move our hands to the level required at the other house, or at least experience a tendency to do so. This movement or tendency to movement is not adjusted to the new conditions; the set has to be counteracted by a fresh movement downwards, and, accompanying this readjustment, there springs into consciousness a vivid impression of the lowness of the handle; which is in this case the more striking because we know that the handle is really in the same position as on the many occasions when we used it before and when it did not appear to be unusually low.

Cases of this sort, involving actual movement, undoubtedly afford the clearest evidence of the contrast-producing effect of an inadequate or distorted set. But there would seem to be no great difficulty in imagining a similar effect to be produced in a purely perceptual set. Suppose it is the height of the ceiling, and not the height of the door handles, which seems to have changed when we return home. In this case, during our absence we have lived, say, in loftier rooms than those of our own house; while thus living, the repeated visual impression of the height of the rooms modifies the set which had been formed by many impressions of the height of the rooms in our home; with the result that, on our return home, the set is no longer adjusted for the assimilation of the visual impressions we receive, and the perception of the familiar room is qualified by an absolute impression of lowness or lack of height.

In a similar way we may regard all contrast effects, qualifying the perception of familiar objects and following upon the perception of similar objects differing in respect of the contrasting feature, as instances of absolute impression determined by modification of a perceptual set; in every case the set being the product of repeated perception of the familiar object; the perfect adjustment of the set to the object being the ground of our feeling of familiarity with the object; and the modification or distortion of the set produced by perception of a similar, though different, object, the ground of an absolute impression qualifying the subsequent perception of the original object.

We are inclined to think that all the phenomena classed together under the heading of Psychological Contrast are but marked cases of this kind of shock—that Contrast is in fact always the result of an inadequate set. If we examine the cases that occur in ordinary life, we find that most of them can be easily explained in this way. There are of course great differences in the nature of the sets concerned, corresponding to the great variety of the mental processes in which contrast effects are found. The sets may be motor dispositions of the simplest kind, or complicated mental dispositions involving a great number of intellectual and emotional factors. Similarly there are great differences in the duration of the sets; they may persist for a few minutes or seconds only, or may endure for years without discoverable change. But these differences do not affect the essential nature of the shock which is caused by the discrepancy between the set and the immediately following impression.

But, if the absolute impression which constitutes Successive Contrast is determined by lack of adjustment of the set formed by foregoing perceptions of similar objects, it seems clear that simple acts of perceptual comparison are conditioned by similar sets. The old-fashioned 'image theory' of comparison is sufficiently exploded and needs no blows from us for its further demolition. The simple fact, obvious to every unprejudiced observer, is that, in comparing in respect of some quality two simple objects of perception, we turn our attention from one to the other, and that, as we perceive the second, the judgment is immediately formed and completed, and only needs to be formulated in words (provided of course that the difference between the two objects is large enough). In intentional comparisons, such as we make in the course of psycho-physical experiment, the subject's intention plays an important part; but this is not essential; for the difference between the similar objects, if it be sufficiently large, may be brought to consciousness immediately on the turning of the attention from one object to the other, without the subject having any intention to compare them. A single perception of the one object suffices, then, to leave in the mind a set which may either assimilate the impression made by a similar object, or may (if the second object differs appreciably) determine awareness of difference. And instances of 'Successive Contrast' seem to be strictly comparable to cases in which, after turning from object A to object B, we turn back to A and become immediately aware of its difference from B. The cases differ only in that in those we call Successive Contrast each object has usually been perceived frequently; first, repeated perceptions of A render the set more enduring and more accurately adjusted to the object; then, repeated perceptions of B modify the set; and lastly, upon renewed perception of A, the distortion of the set produced by perceptions of B determines the absolute impression (of bigness, smallness, brightness, heaviness, etc., according to the quality in which B differs from A).

We are now in a position to consider cases of so-called 'Simultaneous Contrast.' We have to consider two rather different classes. Of the one class the man of medium stature seen beside a tall man and appearing in consequence short, is the classical instance. It seems to us obvious that cases of this class are properly regarded as essentially similar to cases of Successive Contrast. Repeated perceptions of men of medium stature have formed a perceptual set for the assimilation of such impressions; and the perception of the unusually tall man suffices momentarily to distort this set, so that as our attention turns at the next moment to the normal man, our perception of him is qualified by an absolute impression of shortness.

Of the second class of instances of Simultaneous Contrast we may take as the type the one made familiar in textbooks, namely that of two circles of equal size, the one (A) surrounded by smaller, the other (B) by larger circles; or that of two narrow equal spaces, the one (A) bounded by small, the other (B) by large rectangles. These cases also may, we think, be properly regarded as essentially of the nature of Successive Contrast; though this is perhaps less obviously true than in respect of the former class of simultaneous contrasts. First, although the two figures may be presented in the same field of view, we, in order to compare them, turn our eyes and our attention from one to the other. Secondly, it seems clear that the contrast effect is produced only on condition that we perceive the figures analytically, if, that is, our attention singles out the circles (or spaces) A and B from the surrounding figures. If, on the other hand, we regard each group comprehensively as one whole, our comparison of A with B is apt to be influenced by the surrounding figures by way of 'confluence,' so that A, instead of appearing larger than B owing to contrast, appears smaller than B. But on perceiving analytically the group of figures of which A (or B) is a member, we attend to A (or B) and to the figures or areas surrounding it successively, rather than simultaneously; hence, in accordance with the principle of Successive Contrast discussed above, the perception of A is apt to be qualified by an absolute impression of largeness, that of B by one of smallness; and hence we are apt to judge A to be larger than B.

If the view here maintained is correct, it would seem that all

Psychological Contrast may be reduced to one type. In every case Psychological Contrast appears as the result of a distortion and consequent inadequacy of a set, which determines a failure of the assimilative process; the set being one which, had it been perfect, would have produced a complete correspondence between the implicit expectation determined by the set and the ensuing impression. This assimilative process is but another name for judgment, if we use that term in its widest sense. Hence, if judgment were always perfectly correct, Contrast would not occur. Contrast implies a failure or distortion of judgment; and its occurrence implies that we are not perfectly adapted to our environment. But it is also an aspect of the process by which we learn by experience, *i.e.*, of the process of modification of old psycho-physical dispositions by new impressions. Contrast occurs, it is true, only in extreme cases of this process. Our psycho-physical dispositions are probably to some extent modified each time they are brought into play, while it is only the conspicuous failures of adjustment that give rise to the vivid impressions which we describe as contrast effects; but it is just this fact (that it represents in a magnified form a process which is of continual occurrence and of the greatest importance for the growth of mind) that is likely to make Contrast a valuable subject of investigation to the psychologist. It is from this point of view that it seems worth while to put on record the following experiments in Psychological Contrast.

All the experiments recorded below were made in the year 1909, in the Psycho-physical Laboratory at Oxford. There served as subjects, besides the writers, two ladies and one gentleman, to whom our thanks are due for the kind way in which they put themselves at our disposal whenever the pressure of other work permitted them to do so.

In the course of our investigation we endeavoured to obtain contrast effects in the following six operations:—

(1) comparing weights lifted by the hand;

(2) recognising colours;

(3) estimating the length of a straight line when surrounded by areas of different sizes;

(4) estimating the sizes of angles;

(5) judging the rate at which auditory impressions succeed one another (time);

(6) judging the rate at which visual impressions succeed one another (seen motion).

In all cases except (3) Successive Contrast only was investigated. In (3) the Contrast was of course simultaneous.

In all cases of Successive Contrast the general plan of the experiments was the same. A fixed or 'standard' object of medium intensity, magnitude, or quality was chosen, and the impression made by it under ordinary circumstances was compared with that made by it after directing the attention to objects of the same kind but of different intensity, size or quality. Each experiment thus consisted essentially of three stages—(1) judgment upon the standard object; (2) the presentation of the contrast-inducing object; (3) judgment passed upon the standard object under the same external conditions as those of stage (1). Since it is convenient to have names to designate these different stages of the experiment, we have, throughout the paper, referred to the series of observations constituting stage (1) as the 'standard series,' and the object used in this stage as the 'standard object.' Stage (2) we have called the 'contrast-inducing series,' since this part was undertaken for the purpose of inducing a contrast effect when a return was made to the standard object. The objects used in this part of our experiments we have called the 'contrast-inducing objects.' Stage (3), the re-presentation of the standard object, we have called the 'comparison series,' since it is by a comparison of the results of this series with those of the standard series that the Contrast, where it exists, becomes apparent. The object used here is called the 'comparison object,' although it is of course physically identical with the standard object.

We employed the methods both of reproduction and of recognition during our investigation, according as the one or the other seemed most suited to the conditions of the experiment in hand. The results obtained by the two methods appear at first sight to be contradictory. But the contrast effect, when present, must necessarily manifest itself differently by the two methods; and the results obtained by the two methods, therefore, were really in perfect agreement with each other. The reader will understand the reasoning of the following paragraph more easily after reading the descriptions of the experiments.

In the method of recognition, if the contrast-inducing object has been (say) of greater intensity than the standard object, the latter, when again presented, will (supposing contrast to be operative) be judged to be less intense than the original standard; and therefore also, out of a number of objects varying in intensity about that of the standard, a larger number than before will appear less intense than the original standard as remembered by the subject; the effect of the contrast-inducing series is therefore a marked increase of the total number of judgments 'less intense' at the expense of the 'more intense' ones; *i.e.* this is the effect to be expected from the operation of Contrast. By the method of reproduction, however, supposing the subject to have so adjusted the comparison object that it is of the same objective intensity as the standard, it will as before be judged to be less intense; the subject will therefore increase the intensity in order to make it appear the same as the standard. The figures obtained from the comparison series will therefore differ from those of the standard series in the same direction as those of the contrast series; this, which might at first sight seem to indicate the opposite of Contrast, is really only the natural revelation of Contrast by this method.

II. INVESTIGATIONS INTO THE OCCURRENCE OF PSYCHOLOGICAL CONTRAST EFFECTS IN

(i) *Judgment of lifted weights.*

I. In our observations on lifted weights, as well as in several other of our experiments, it was our original intention to practise with a standard stimulus until we had acquired something approaching an absolute memory for the stimulus of this intensity, then to practise for a given time with a stimulus of considerably higher or lower intensity, and finally to return to the original standard and find out what effect the practice with the contrast stimulus had made upon the memory of the standard. It was, however, found impossible to acquire anything like an absolute memory for a standard weight within a reasonable time. Although we practised lifting a standard weight of 100 grams and several other weights varying between 80 and 120 grams for several weeks, we found that, except during the first two or three days, there took place little or no improvement in our ability to pick out the standard weight from among the others. We therefore abandoned the attempt to acquire an absolute memory of the standard before beginning our contrast series and decided to adopt a procedure which should dispense with the necessity of long preliminary practice with the standard.

The simplest possible procedure seemed to be to make a number of comparisons between the standard weight (remaining constant throughout the experiment) and a series of weights varying slightly from the standard in both directions, interpolating in each case between the two weights to be compared a contrast-inducing weight varying more widely from the standard. This method we adopted in one of the series recorded below. The weights used in this and in the following series were of the Galton cartridge pattern, indistinguishable from each other in appearance except by small figures on one side, which indicated their weight in grams. The subject sat at the end of a table, with a small screen in front of him, shutting out most of the table from his view. Behind this screen were placed the weights to be used in the experiment, only such weights as were needed for the comparison to be made at the moment being placed within view of the subject (the figures on them turned so as to be invisible to him). The experimenter was so seated at the corner of the table that he could make the necessary changes of the weights with his left hand while he recorded the judgments of the subject with his right hand. To ensure the weights being lifted to the same height on every occasion, a string was stretched between two uprights nine inches above the table and immediately over the weights to be lifted. The subject lifted the weight with a free continuous movement, until his hand touched the string; whereupon he lowered the weight to its former position on the table.

In the first series recorded three weights were lifted in the following order for every judgment made:—(1) A standard weight of 50 grs.; (2) one of three contrast-inducing or 'contrast' weights of 40, 80 and 120 grs.; (3) one of the following 'comparison' weights, which had to be compared with the standard—44, 46, 48, 52, 54, and 56 grs. The subject had to say whether the third weight lifted was lighter or heavier than the first. In the following table the first column represents the magnitude of the 'comparison' weight—the weight compared with the standard in each case, the next two columns the number of times that the corresponding weight was judged lighter or heavier respectively than the standard when the intermediate 'contrast' weight was 40 grs., the next two columns the same figures when the 'contrast' weight was 80 grs., and the last two columns the same with the 'contrast' weight of 120 grs. Each of the 'comparison' weights was lifted ten times with each of the 'contrast' weights; thus 60 judgments were made with each of the latter and 180 in all. Both the 'contrast' weights and the 'comparison' weights were presented to the subject in irregular order. It will be seen that the intervening 'contrast' weight has a decided influence on the judgments of comparison and that this influence is of the nature of a contrast effect, the number of 'lighter' judgments being greater when the 'contrast' weight is heavier than the standard and smaller when it is lighter than the standard. The contrast effect is greater as the difference between standard and 'contrast' weight is larger; the contrast weight of 120 grs. produces a greater effect than that of 80 grs.

Subject: J. C. F.

'Contrast' weight	40		80		120	
'Comparison' weight	L	H	L	H	L	H
44	8	2	9	1	9	1
46	8	2	9	1	10	0
48	8	2	6	4	9	1
52	0	10	6	4	5	5
54	1	9	2	8	3	7
56	1	9	2	8	2	8
Totals	26	34	34	26	38	22

In order to simplify the task of the subject, in our later experiments we adopted a slightly different procedure. Instead of lifting three weights for each judgment, and comparing the third with the first, the subject now lifted four weights before expressing each judgment, the order being as follows:—(1) standard of 100 grs.; (2) one of four 'contrast' weights of 80, 90, 110, and 120 grs. respectively; (3) the standard again; (4) one of the following nine 'comparison' weights:—94, 96, 98, 99, 100, 101, 102, 104 and 106. The subject had to compare (4) with (3), and was allowed to express his judgment in any one of three forms, viz. that (4) was 'lighter,' 'heavier' or the 'same' as (3). The subject knew that (1) and (3) were of the same weight (the same cartridge being used in fact for both, the experimenter merely substituting (4) for (2)) and was also aware of the general order of the experiment, though of course ignorant which particular (2) or (4) was to be given in each experiment. The following table is arranged similarly to the preceding one, there being now, however, under the heading of each of the contrast weights three columns for each of the comparison weights (for 'lighter,' 'heavier' and 'the same' judgments respectively). Each of the 'comparison' weights was lifted twenty times with each of the 'contrast' weights, there being thus 180 judgments with each of the latter and 720 in all. The general conditions of the experiment were similar to those of the preceding one, though, owing to the much larger number of judgments made, it was only completed after several sittings.

Subject: Miss S.

'Contrast' weight	80			90			110			120		
'Comparison' weight	L	H	S	L	H	S	L	H	S	L	H	S
94	11	7	2	13	6	1	17	3	0	16	3	1
96	9	8	3	11	7	2	14	1	5	15	3	2
98	9	9	2	7	10	3	12	7	1	14	4	2
99	4	11	5	9	7	4	12	6	2	14	2	4
100	4	14	2	6	8	6	8	7	5	11	6	3
101	4	10	6	5	10	5	8	11	1	9	4	7
102	2	16	2	5	11	4	7	8	5	6	9	5
104	3	12	5	2	13	5	4	10	6	5	11	4
106	0	17	3	2	15	3	7	9	4	3	14	3
Total	46	104	30	60	87	33	89	62	29	93	56	31

For the purpose of studying more easily the contrast effects manifested in this experiment, we have thought it permissible to eliminate the 'same' judgments by distributing them among the 'lighter' and 'heavier' in the following manner:—those which occurred when the 'comparison' weights were lighter than the standard were classed with the 'heaviers,' since they represent an error in the same direction as that which occurs when the same weight is pronounced 'heavier' than the standard, and, so far as they are due to Contrast at all, may be supposed to express only a somewhat less marked effect of the same cause that leads to the 'heavier' judgments. Similarly, the 'same' judgments made when the 'comparison' weights were heavier than the standard have been added to the total number of 'lighter' judgments. The 'same' judgments made when the 'comparison' weight was the same as the standard (100 grs.) have been equally divided between the 'lighter' and 'heavier.' When this is done, we obtain the following figures:—

'Contrast' weight	80		90		110		120		Total	
	L	H	L	H	L	H	L	H	L	H
	63	117	80	100	107½	72½	113½	66½	364	356

It will be seen that the contrast effect brought out in the first experiment is manifested again under the new conditions. For the different proportions of 'lighter' and 'heavier' judgments in each group of 180 experiments (with each of the 'contrast' weights) seem to be the expression of the contrast-inducing influence effected by the lifting of the 'contrast' weights. It is to be noted also that here again the magnitude of the contrast effect is greater, the greater the difference between the standard and the 'contrast' weight.

At the same time as the above experiment, a shorter series of observations under precisely the same conditions was made with another subject. In this series nine sets of judgments were made with each of the 'comparison' weights, there being thus 81 judgments with each of the 'contrast' weights and 324 in all. The results are recorded below.

Subject: Miss K.

	80			90			110			120		
	L	H	S	L	H	S	L	H	S	L	H	S
94	5	3	1	6	2	1	8	0	1	8	0	1
96	1	5	3	2	6	1	8	1	0	6	1	2
98	0	7	2	1	6	2	6	2	1	6	3	0
99	4	3	2	6	3	0	5	0	4	6	3	0
100	1	7	1	1	4	4	1	2	6	3	3	3
101	3	4	2	1	8	0	3	3	3	6	1	2
102	1	7	1	1	6	2	1	6	2	5	3	1
104	0	7	2	3	5	1	3	4	2	3	4	2
106	0	8	1	1	8	0	0	6	3	4	4	1
Total	15	51	15	22	48	11	35	24	22	47	22	12

The 'same' judgments being treated as before, we get the following figures:—

80		90		110		120		Total	
L	H	L	H	L	H	L	H	L	H
21½	59½	27	54	48	33	54½	26½	151	173

It will be seen that the general tendency of this series is in complete accordance with that of the preceding one. For the sake of convenience of comparison, we have reduced the results of both series to percentages, as follows:—

	80		90		110		110		Total	
	L	H	L	H	L	H	L	H	L	H
Miss S.	35	65	43·4	56·6	59	41	65·3	34·7	50·9	49·1
Miss K.	26·5	73·5	33·3	66·7	59·9	40·1	67·3	32·7	44·2	55·8

We were able to make one other similar, but considerably shorter, series of observations, in which one of the writers served as subject. He, of course, unlike the other subjects, was aware of the nature and purpose of the experiments. The procedure was the same as that of the two preceding series, but there were only three 'contrast' weights, viz.:—60, 110 and 120 grs.; and seven 'comparison' weights, viz.:—96, 98, 99, 100, 101, 102, and 104 grs.

Subject: W. McD.

	60			110			120		
	L	H	S	L	H	S	L	H	S
96	1	8	1	2	4	4	2	5	3
98	4	4	2	5	4	1	6	3	1
99	1	7	2	2	4	4	6	3	1
100	0	7	3	2	7	1	3	5	2
101	0	8	2	1	7	2	4	5	1
102	2	4	4	3	5	2	3	3	4
104	1	8	1	0	8	2	4	4	2
Total	9	46	15	15	39	16	28	28	14

The 'same' judgments being treated as before, we get:—

60		110		120		Total	
L	H	L	H	L	H	L	H
17½	52½	21½	48½	36	34	75	135

The results of this series are somewhat obscured by the very large constant error in favour of the 'heavier' judgments, but the Contrast is still seen to work in the same direction as in the earlier series, the effect of the Contrast with the weight of 120 grs. counteracting approximately the tendency to this constant error.

From the results of the four sets of observations here recorded, we seem justified in concluding that a very considerable contrast effect is brought about in the procedure used by us.

A more obvious way of studying the contrast effects in lifting weights would perhaps have been to have set up a definite motor disposition, as in the methods adopted by Laura Steffens and others, and to have observed the effect of this disposition, on a subject being asked to pick out a weight equal to a standard lifted before the disposition was set up. As was mentioned at the beginning of this section, we refrained from adopting this procedure, owing to our want of success in acquiring anything like an absolute memory for the standard weight. It is probable, however, that interesting results might be obtained by this method even without long preliminary practice with the standard weight. The experiments to be described below were mostly conducted in this way and not by the series of individual comparisons employed in the experiments just described; and (except in one case) we were always successful in establishing the existence of contrast by this method. Moreover, the experiments made on motor dispositions show that these dispositions manifest themselves as contrast effects; thus when a 'light-heavy' disposition is set up by lifting a number of times in succession first a light and then a heavy weight, if two equal weights be then lifted, the second will seem lighter than the first. This phenomenon may seem comparable to that which occurs in the last three of our own experiments just recorded; it seems quite possible that the effect produced on the subject's judgment when comparing the third and fourth weights lifted may depend on conditions of the same nature as (though of course of lower degree than) the disposition set up by lifting two different weights in the same order a large number of times, as in the experiments of Laura Steffens. Against this view, however, it might be urged (1) that it is unlikely that only one lifting of the contrast weights would set up a disposition strong enough to produce the quite marked contrast effect manifested in our experiments; (2) that, even if this were possible, the different dispositions set up at each lifting of the 'contrast' weights, owing to these being used in irregular order, would tend to annihilate one another, so that no one of the four possible dispositions would, at any moment, sufficiently preponderate over the others to account for our results. It seems likely that future more elaborate experimentation, by measuring the rapidity with which the weight is lifted in each case, might be able to afford definite evidence one way or the other. If the presence of a motor disposition is definitely disproved, we shall of course be driven to assume a more central origin of the contrast effect observed, in which case it would be more strictly comparable to the Contrast manifested in our other experiments. Moreover, even if the presence of a motor disposition is possible in the three last experiments recorded, it is difficult to account for the results of the first experiment by this means, since here we should expect, if anything, a disposition corresponding to the *three* weights lifted for each judgment made, and this would tend to hide rather than to accentuate any influence of the second weight on the third. Here again, however, more elaborate work should enable us to decide whether the contrast effect is to be attributed to some difference in the effort put forth in lifting the weights or to purely central conditions.

(ii) *Recognition of colours.*

Although Helmholtz's explanation of colour contrast, as due entirely to an error of judgment, has long been abandoned by every authority in favour of the view that the phenomenon is to be accounted for by peripheral changes which modify the qualities of sensation, it has been held by some supporters of a supposed 'general law of contrast' that, in addition to the generally recognised Physiological Contrast, some factor of more central origin may also play a part in Colour Contrast. It occurred to us that this assumption might easily be made the object of experimentation. After some preliminary experiments, we decided upon the following method as likely to yield the best results in a short time.

The subject sat at a given distance in front of a table on which was placed a Marbe colour-wheel, provided with a red and a blue paper of good saturation and brightness, and driven by a water motor at a speed sufficient to produce complete fusion of the two colours. A standard mixture of 180° of both colours was presented to the attentive observation of the subject for ten seconds. The subject then closed his eyes, while the experimenter altered the relative proportion of the two discs, making the mixture widely different from the standard. The subject then again regarded the disc, while the experimenter again slowly, but at a rate varied in each experiment, altered the proportion of the two discs in the direction of the production of the standard colour, the movement being stopped when the subject indicated that, in his opinion, the mixture was of the same quality as the standard. The proportion of the two colours was then read off on the arbitrary scale with which the instrument is provided. After the subject had made ten attempts at reproducing the standard—the standard being presented to him before each attempt—he made a certain number of similar attempts without seeing the standard, and afterwards made a third series of reproductions, this time with a contrast-inducing standard of different quality; this contrast-inducing colour, which contained a distinctly greater amount of blue or red than the old one, was presented before each reproduction as in the former series. A pause, usually of about 15 minutes, but varying between 10 minutes and 1½ hours, was then made; after which the subject renewed the attempt to reproduce the original standard without having it again presented to him. In each experiment we made therefore four series of observations, of which the first served merely to enable the subject to learn to reproduce the standard with fair accuracy; while the second constituted the standard series; the third the contrast-inducing series; and the fourth the attempt to return to the original standard, this being the comparison series. It was hoped that if there were any considerable 'psychological' contrast effect exerted by the third series, it would be brought to light by a comparison of the fourth with the second series. The long interval between the third and fourth series was interposed with the object of abolishing any colour adaptation or fatigue; this would almost certainly have entirely disappeared in the time allowed. The disc was evenly illuminated by a single electric lamp throughout each experiment; and, in order to avoid any results of altered adaptation, the subject did not leave the room (which was artificially illuminated) during the time occupied by the four series and the interval between the third and fourth.

In the following table the figures refer to the arbitrary scale on the Marbe wheel. On this scale 120 corresponded to 180° of either colour and constituted our original standard, which was the same in all our experiments. The figures above or below 120 represent larger and smaller amounts of red respectively. The left-hand half of the table gives the results when the 'contrast' colour contained more red, the right-hand half those when it contained less red, than the standard. The first column in each half gives the results of the standard series (the second of the four series mentioned above) in terms of the proportion of red (measured on the arbitrary scale) chosen by the subject as giving a purple of the same quality as the standard; the second column gives the proportion of red in the new (contrast-inducing) standard (the third of the above series); the third column the interval (in minutes) that was allowed to elapse before the making of the 'comparison' series, the results of which are recorded in the fourth column in the same terms as those of the first column. In the first and fourth columns the higher numbers give the averages of the reproductions made by the subject, the lower numbers in brackets the mean variations. In some cases several experiments with different contrast standards were made without the original standard being shown to the subject except in the first series of the sitting. Here there is only one set of figures to record in the first column for two or more in the succeeding columns. On a few occasions two comparison series were made for one contrast series, the first of these comparison series being made after a short, the second after a longer interval; when this was done the figures in the 'time' column represent the interval measured from the end of the contrast series in both cases.

		'Contrast' colour redder than standard.				'Contrast' colour bluer than standard.			
No. of experiment	Initials of subject	Value of Red in standard series	Value of Red in contrast colour	Interval in minutes	Value of Red in comparison series	Value of Red in standard series	Value of Red in contrast colour	Interval in minutes	Value of Red in comparison series
1	J. C. F.	133·4	230	40	136·6	—	—	—	—
		(3·2)	—	—	(1·3)	—	—	—	—
2	J. C. F.	—	—	—	—	124·2	60	10	114·1
		—	—	—	—	(4·6)	—	—	(2·7)
3	J. C. F.	119·3	200	10	134·0	—	—	—	—
		(2·7)	—	—	(3·4)	—	—	—	—
		—	—	60	126·7	—	—	—	—
		—	—	—	(1·7)	—	—	—	—
4	J. C. F.	—	—	—	—	126·5	60	15	113·5
		—	—	—	—	(4·1)	—	—	(2·9)
5	J. C. F.	—	—	—	—	129·2	60	15	118·7
		—	—	—	—	(3·6)	—		(3·7)
		—	—	—	—	—	—	90	113·2
		—	—	—	—	—	—		(3·0)
6	J. C. F.	118·8	200	10	134·7	—	—	—	—
		(3·4)	—	—	(2·2)	—	—	—	—
		—	—	60	122·2	—	—	—	—
		—	—	—	(3·6)	—	—	—	—
7	J. C. F.	119·7	170	15	132·0	—	—	—	—
		(2·7)	—	—	(2·7)	—	—	—	—
8	J. C. F.	118·8	170	15	125·2	—	70	15	113·2
		(1·4)	—	—	(3·8)	—	—	—	(3·0)
		—	220	15	140·6	—	20	15	113·2
		—	—	—	(6·4)	—	—	—	(2·6)
9	Mr B.	119·6	220	15	160·2	—	20	15	97·2
		(2·8)	—	—	(3·8)	—	—	—	(8·2)
10	Mr B.	134·8	170	15	145·0	—	70	15	116·0
		(4·2)	—	—	(4·4)	—	—	—	(4·8)
11	Miss S.	108·8	220	10	128·6	120·4	20	60	104·8
		(0·6)	—	—	(1·2)	(3·2)	—	—	(2·6)
12	Miss S.	116·6	170	15	116·8	—	70	60	118·2
		(2·4)	—	—	(1·8)	—	—	—	(0·6)

It will be seen from the table that out of the 12 sets of observations recorded, all but one—the last—show very clearly the existence of a contrast effect. We are unable to suggest an adequate explanation of this single negative result; it may be observed that the subject stated that she was in a condition of considerable emotional excitement on the occasion of the earlier of the two experiments made with her (which seems to show a decided contrast effect) while this was not the case during the second experiment (in which no contrast was manifested). The subject (J. C. F.), on whom most of the observations were made, was, during the later experiments, aware of the general tendency manifested in the earlier ones (though he had no knowledge of the actual figures obtained), so that it is conceivable that suggestion may have played some part in the results obtained in experiments 6–8. This, however, cannot have been the case with the other subjects, who were entirely ignorant both of the purpose of the experiments and of the results hitherto obtained. As regards the influence of the contrast series, our results may perhaps be held to show that (as in the case of our experiments with weights) the greater the difference between the 'contrast' colour and the standard colour, the greater the contrast induced. Experiments 8, 9 and 10 afford most information on this point. In 9 and 10 the rule holds good when the contrast colour was both redder (left side) and bluer (right side) than the standard. In experiment 8 it holds good only in the former case. With regard to this point it is perhaps worth mentioning that the very intense red of the contrast stimulus 230 produced some degree of emotional excitement in the subject (J. C. F.), which was absent with the contrast stimulus of 200.

Our observations in which two comparison series were made at different intervals after the contrast series are insufficient to afford definite information as to the speed with which the contrast effect wears off. In experiments 3 and 6, in which an interval of 10 minutes elapsed between the contrast series and the first comparison series, and an interval of one hour between the contrast series and the second comparison series, we seem to be able to observe the gradual waning of the contrast effect. In experiment 5, on the other hand, there seems, curiously enough, to be an increase of the effect after a still longer interval of 1½ hours.

We think that, taken as a whole, the above experiments show beyond all doubt the occurrence of a successive colour-contrast effect, presumably of central origin; this seems to be quite distinct from the ordinary Physiological Contrast, since the effects persist for a very considerable time, being well marked after an interval of 15 minutes, and apparently not exhausted after the lapse of an hour or more. The results may be summarily stated in the following terms:—The subject's impression of, or setting for, the standard colour (formed during and persisting after the initial and the standard series) was in every case but one modified by the contrast-inducing series in the direction of assimilation to the contrast-inducing colour, so that in the comparison series he judged the standard colour as though it had been modified in the opposite direction.

(iii) *Judgments of length of lines.*

Is our judgment of the length of a straight line in any way affected by the size of the area surrounding it? Our investigation of this problem was carried out under the following conditions:—The subject sat in the dark room of the laboratory opposite to and 3 metres distant from a window opening into an adjoining room. In the window was placed a thick sheet of milk glass, and over this was fixed a shutter containing an adjustable Aubert diaphragm, by means of which the area of the window exposed could be varied to any desired extent; this area, whatever its size, was always of the shape of a square, its diagonals being horizontal and vertical respectively; the variable length of the side of this square was indicated on a millimetre scale. In front of the window and a few cm. from it, on the side remote from the dark room, was placed a large metal screen with a horizontal slit 1 mm. wide, the length of which could be adjusted at will by the operator by means of a sliding shutter. Immediately opposite this slit and at a distance of 3 metres from it was placed an electric lamp, the light from which, passing through the slit, fell as a bright line on the milk glass in the window of the dark room, the rest of the surface of the glass being illuminated only by diffused daylight. To the subject, therefore, sitting in the dark room, there was presented a fairly bright square, upon which was defined a much brighter line; the size of the square being determined by the adjustment of the movable shutter, the length of the line by that of the horizontal slit. The position of the line on the square could be altered by altering the position of the electric lamp in the adjoining room; and this was done frequently throughout the experiments, in order that the subject's estimate of the length of the line might not be influenced by the distance between the end of the line and the edge of the square. As regards the presentation of the straight line, two distinct procedures were adopted by us. In our earlier experiments the method was the same as that of the experiments recorded in the last section. Each set of observations was preceded by a practice series, in which the subject endeavoured to reproduce the standard line that had been presented to him for a few seconds before each attempt. As in the former experiments, the actual adjustment was made by the experimenter, who slowly increased the length of the horizontal slit (by means of a screw rack) until the subject gave the signal to stop; whereupon he read off the length of the opening on a scale provided for the purpose. After 10 of these practice reproductions, the actual experiments were begun. In all those recorded below, observations were made with squares of seven different sizes, the square standing midway in these seven as regards size being identical with that with which the practice experiments had been made, and which we shall therefore call the standard square. At each sitting five reproductions were usually made with each size of square, the squares being presented in irregular order determined beforehand by the experimenter. We record below some of the results obtained under these conditions. In the tables the first row of figures represents the length (in cm.) of the sides of the different squares used (the same throughout our experiments); the second row the average length of the lines produced by the subject, with the corresponding size of square; the third row the mean variation for the same. The first table gives the results of 10 reproductions with each size of square (2 sittings), the standard line being 7 cm. in length, the side of the standard square being 17·5 cm.

Subject: McD.

	25	22·5	20	17·5	15	12·5	10
Av.	7·53	7·23	7·15	7·00	7·06	6·90	6·60
m.v.	·43	·25	·16	·40	·19	·14	·50

It will be seen that the size of the surrounding square appears to exercise a distinct and constant influence upon the length of the line produced by the subject, and that (having regard to the method of reproduction here employed) this influence takes the form of a contrast effect, for where the square background is larger than the standard square the line given by the subject as equal to the standard line is longer than the standard (and conversely); that is to say the standard line presented upon the larger square appears shorter than when presented on the standard square (and conversely). This effect comes out particularly clearly if we pool the results obtained with the three squares larger than the standard square, and compare them with those obtained with the squares smaller than the standard, in which case we obtain as averages 7·30 and 6·85 respectively.

Encouraged by this result, we made two longer series under precisely similar conditions, except that the standard line was 5 instead of 7 cm. in length. Four sittings were made at each of these series, 20 judgments being recorded for each size of square.

Subject: McD.

	25	22·5	20	17·5	15	12·5	10
Av.	5·67	5·18	5·24	5·23	5·17	4·74	5·20
m.v.	·45	·26	·37	·26	·30	·27	·35

Subject: Miss S.

	25	22·5	20	17·5	15	12·5	10
Av.	5·98	5·96	5·36	5·78	5·54	5·30	5·23
m.v.	·27	·33	·29	·28	·35	·23	·49

It will be seen that the results of these experiments, though less regular than those of the first, bear out the general tendency there manifested. This comes out clearly when we compare the average of the results with the larger squares with the average for the smaller squares, in which case we obtain as averages, for McD. 5·36 and 5·04 and for Miss S. 5·77 and 5·36 respectively.

Thinking that perhaps the contrast effect might be modified or abolished, if the subject had the standard constantly before him during the experiment, instead of having to work from his memory of the standard line seen only before the experiment began, we carried out our later experiments under slightly altered conditions. A second window between the dark room and the adjoining room was now opened and covered with a plate of milk glass. Over this on the dark room side was placed a stout sheet of millboard, with an aperture 17·5 cm. square, in a position corresponding to that of the aperture of the adjustable shutter on the other window. A short distance in front of the new window, on the opposite side, was fixed another sheet of millboard, with an aperture of the same width as the horizontal slit in the metal screen placed before the other window, and 5 cm. in length. A second electric lamp was then placed before this new aperture. The arrangement of the new window was, therefore, exactly similar to that of the old one, when the opening of the shutter and the horizontal slit were both of standard size (17·5 sq. cm. and 5 cm. respectively). The subject shifted his position slightly, so as to be midway between the two windows, and care was taken that the illumination both of the horizontal slit and of the rest of the window was of the same intensity in both cases.

In our experiments under these new conditions the procedure was exactly similar to that of our earlier experiments, except that the operator increased the length of the horizontal slit rather more slowly than before, in order that the subject might be able to look from one window to the other as often as he liked during the experiment.

The following are the results obtained under the new conditions, both series being of the same length as the last two (*i.e.* 20 reproductions for each size of square):—

Subject: McD.

	25	22·5	20	17·5	15	12·5	10
Av.	5·19	5·26	5·06	4·87	4·70	4·69	4·56
m.v.	·19	·18	·15	·16	·15	·18	·18

Subject: Miss S.

	25	22·5	20	17·5	15	12·5	10
Av.	5·28	5·35	5·15	5·03	4·89	4·78	4·54
m.v.	·17	·20	·20	·16	·24	·10	·15

	McD.	Miss S.
Average for three squares larger than standard	5·17	5·26
„ „ „ „ smaller „ „	4·65	4·74

The above figures indicate that the contrast effect is just as strong here as in the earlier series. We may, therefore, conclude that, in this case at any rate, the interval between the presentation of the standard and its reproduction is of comparatively little importance, so far as the contrast effect is concerned; and from this we may perhaps assume further that the degree of clearness of the memory-image, when one is present and in so far as its clearness is dependent on the lapse of time since the original impression, is probably not a principal factor in contrast.

It will be noticed that in both of the above series, if we exclude the results obtained with the largest square, the figures form an orderly sequence, the length of the line produced increasing with each increase of the size of the square. Since it is fairly certain that in every case of contrast there must be an optimum magnitude of the difference between the contrast-inducing object and the standard object, it seems probable that, in this case, the 25 cm. square may be above the optimum size. In the hope of obtaining fresh evidence on this point we carried out a number of further observations, the results of which are recorded in the following table.

			25	22·5	20	17·5	15	12·5	10
1	Miss K.	Av.	5·15	5·19	5·03	5·20	4·97	4·88	4·61
		m.v.	·20	·07	·11	·15	·15	·16	·09
2	Miss K.	Av.	5·46	5·62	5·68	5·26	5·05	5·22	5·03
		m.v.	·25	·18	·18	·21	·17	·20	·29
3	Mr B.	Av.	4·89	4·63	4·97	4·89	4·79	4·88	4·63
		m.v.	·23	·19	·18	·23	·11	·19	·16
4	J. C. F.	Av.	5·15	5·19	5·03	5·20	4·97	4·88	4·61
		m.v.	·20	·07	·11	·15	·15	·16	·09
5	J. C. F.	Av.	5·08	5·27	5·09	5·03	4·99	4·84	4·69
		m.v.	·12	·09	·09	·15	·15	·14	·09

In each of the five series 10 observations were made with each size of square, except in the fourth, when 15 were made. The procedure was the same as in the earlier experiments, except that (1) in the second and fifth series the horizontal line was gradually decreased in length, instead of being increased as in all our other experiments, (2) the size of the square on the second window, on which the standard line was projected, was reduced from 17·5 sq. cm. to 10 sq. cm. The results of these series taken individually are, in some cases, less conclusive than those of the earlier series, this being probably due, in part at any rate, to the smaller number of observations made in each case. Nevertheless, the general tendency towards a steady increase in the length of the reproduced line with each increase on the rise of the surrounding square up to 22·5 sq. cm. and a subsequent slight decrease in the length of the line on the square of 25 sq. cm. is brought out fairly clearly. The same tendency is even more manifest if we pool the results of the five series, in which case we obtain

25	22·5	20	17·5	15	12·5	10
5·146	5·18	5·16	5·116	4·954	4·94	4·714

These results seem thus to corroborate in every respect those of the earlier series as regards the existence of a quantitative relation between the magnitude of the contrast effect and the size of the contrast-inducing square.

Before concluding the experiments on this subject, we made two series of observations with momentary exposures of the comparison line, the method of recognition being, of course, employed in this case. The line was exposed for a small fraction of a second, by means of a tachistoscopic shutter placed in front of the electric lamp, the light from which, passing through the horizontal slit, produced the bright line on the window. The rest of the window was continuously illuminated as before. Only two sizes of square were used in this case, viz., the extreme sizes of 10 and 25 cm. The results obtained under these conditions were quite in harmony with those obtained by the reproduction method, and, although the observations made were few in number, the contrast effect was manifested very clearly on both occasions on which we tried this experiment. On the first occasion, the results with the 10 cm. square seemed to show that a 'comparison' line of 5 cm. appeared most nearly equal to the standard line; those with the 25 cm. square, however, indicated that the 'comparison' line had to be increased to 5·5 cm. before it appeared equal to the standard. It is interesting to compare, in the two cases, the judgments made with a 'comparison' line of objectively the same length as the standard line. On the 10 cm. square it was judged 'equal' four times and 'shorter' twice, while on the 25 cm. square it was judged 'shorter' six times. On the second occasion, there were chosen by the subject as equivalents of the standard line of 5 cm., a line of 4·6 cm. on the 10 cm. square, and one of 5·1 cm. on the 25 cm. square; the 'comparison' line objectively equal to the standard line being judged once 'equal' and four times 'longer' in the first case, and three times 'equal' and twice 'shorter' in the second case. These figures show, we think, quite clearly, that we are justified in regarding the effects manifested in our experiments made by the reproduction method as instances of Contrast.

Taken as a whole, the observations recorded in this section show, beyond reasonable doubt, that, under certain conditions at any rate, the apparent size of a line is influenced by the size of the surrounding area, and that this influence is in the direction of Contrast, increase of the size of the square background leading to an apparent decrease in the size of the line, and *vice versa*. This influence of the surrounding area is brought out by the introspections of some of our subjects, as well as by the figures obtained. Thus, according to the introspective account given by one of us: "the judgment was determined, or at least influenced, by an absolute impression of smallness or bigness of the line, which in turn was determined by its size-relation to the square." Another of our subjects imagined the fixed standard line to be superposed upon the 'contrast' square and judged the length of the changing 'comparison' line by its proportion to this image. In both cases it is evident that the size of the surrounding area played an important part in the reproductions of the line made by the subject.

(iv) *Judgment of width of angles.*

Early in the course of our experimentation we began to make observations on the estimation of the width of angles; and indeed, at the beginning of our work, we anticipated that this would prove one of the easiest methods by which contrast effects could be obtained and studied.

The apparatus used by us consisted of a black ebonite disc 30 cm. in diameter, marked off into degrees on one side, while on the other side, it was provided with two radii, one consisting of a white line painted on the black surface, the other of a movable white thread. This thread was attached at its outer end to a metal scale (marked off into degrees to correspond to those on the disc) which, being provided with a groove, could be made to slide easily over the circumference of the disc; it was thus possible to adjust the thread so as to make any desired angle with the white line. The disc was held by a clamp in front of the subject, and well within his reach, so that he could adjust the angles himself. The face of the disc marked off into degrees was turned away from him and towards the experimenter, who read off on the scale the width of the angles produced by the subject. The procedure adopted was similar to that of the experiments on Colour Contrast already reported, at least four series of observations being made for each experiment, viz.: (1) a series of reproductions of the standard angle, the standard being presented before each attempt; (2) a series of attempts to reproduce the standard angle without the standard being again presented; (3) reproductions of a contrast-inducing angle presented before each attempt; (4) a further series of attempts to reproduce the standard angle. There being here no Physiological Contrast to take into consideration, no long interval between (3) and (4) (as in the experiments on Colour Contrast) was needed.

Under these conditions we made a very considerable number of observations with standard and 'comparison' angles of varying widths; but with neither of the two observers who took part in these experiments do the figures obtained seem to indicate any tendency to vary in any particular direction under particular circumstances. This result was borne out by a few observations subsequently made with slightly different methods; so that we seem justified in assuming that under the conditions described the estimation of the width of angles is not affected by Contrast to any measurable extent.

(v) *Judgment of duration.*

In our experiments on the influence of Contrast in the appreciation of time and rate of motion, although several slightly different procedures were adopted, we were unable to continue any one of them long enough to obtain results of much value in themselves. Such observations as we record, therefore, we regard as being of interest in so far as they seem to suggest methods for future experiment, rather than on account of anything they themselves may seem to prove.

As regards time-contrast, our observations took the form of estimating the rate of succession of auditory impressions caused by the beat of a metronome. In most cases the reproduction method was employed, the subject imitating the beats of the metronome by tapping on a reaction-key, the number of taps in a given time being counted by the experimenter. The standard rate throughout was 100 beats per minute, the 'contrast' rates employed being 152 and 40 beats per minute.

We give below the results obtained with one subject (who had previously had considerable practice in the reproduction of the standard rate of 50 per half-minute) on two successive days, on which we adopted the following procedure:—One of the 'contrast' rates was given on the metronome for 30 secs., the subject keeping time with the beat by tapping on the reaction-key. The metronome was then stopped, the subject, however, continuing to beat for another 30 secs. at what he estimated to be the same rate as before. After an interval of 10 secs. the subject endeavoured to reproduce the standard rate of 50 per half-minute. Then, after another interval of 30 secs., the other 'contrast' rate was given and reproduced. Finally, after an interval of another 10 secs., the subject made a second attempt to reproduce

Subject: W. McD.

		Reproduction during ½-minute			
		of rate 76	of standard rate (50)	of rate 20	of standard rate (50)
1st day	Av. in 30″	76	50·4	21	45·1
	m.v.	1·3	3·2	0·8	2·5
2nd day	Av. in 30″	75·8	52	20·9	46·6
	m.v.	1·1	1·5	1·8	4·1

the standard. Each reproduction lasted 30 secs. The whole series of four reproductions (one of each of the 'contrast' rates and two of the standard rate) was repeated six times on both days, on the first day the faster 'contrast' rate (76 per half-minute), on the second day the slower rate (20 per half-minute) being given first in each series. The standard itself was not given to the subject during the actual experiment, since he had already had very considerable practice with it during numerous preliminary observations.

The following figures obtained from the same subject represent the results of a more prolonged series of observations extending over twelve days. The procedure adopted on each day was as follows:—At the beginning of each sitting, the standard (which had not been previously heard by the subject on that day) was reproduced twice. Two reproductions of one of the 'contrast' rates (the fast and the slow one being given on alternate days) were then made, the rate itself being given on the metronome immediately before the reproduction. The subject then immediately again reproduced the standard, and, after an interval of a minute, made a fourth reproduction of it. A fifth and final reproduction was made after a further interval of half an hour, and at the end of each day's experimentation the subject was given one half-minute's practice with the standard, keeping time with the beat of the metronome. Each period of presentation or of reproduction, as before, lasted 30 secs. In the following table the figures for the two standard series are amalgamated, as the difference between the two was in all cases very small. The reproductions of the 'contrast' rates are not given, since, for our present purpose, they are without interest in themselves.

Subject: W. McD.

Reproduction of standard (50 per ½-min.)	Contrast rate 76 per ½-min.		Contrast rate 20 per ½-min.	
	Av.	m.v.	Av.	m.v.
Before contrast rate	52·76	2·35	50·25	3·06
Immediately after contrast rate	59·08	5·25	47·08	2·92
1½ mins. ,, ,, ,,	61·91	2·42	46·50	2·83
30 mins. ,, ,, ,,	54·40	1·68	47·66	2·00

The results confirm those of the previous series, in showing that both the quicker and the slower 'contrast' rates influence the reproduction of the standard rate by way of Contrast. A comparison of the reproductions made immediately and shortly after the contrast series with that made after a further interval of half an hour seems to indicate that the Contrast, though still affecting the latter series, is considerably less strong after the interval, though our experiments are scarcely numerous enough to prove this conclusively.

In the following experiments a slightly different procedure was adopted. As the subjects had not, as in the previous case, already enjoyed considerable practice with the standard, it was necessary to commence each sitting by allowing the subject to listen to the standard during a half-minute. After reproducing the standard, the subject then listened to and reproduced one of the 'contrast' rates in a similar way. She then reproduced in succession the standard and the contrast rates, without again hearing either of them. Each reproduction lasted 30 secs., as before, and the fast and slow contrast rates were given on alternate days. It was unfortunately impossible to get more than eight sittings with one subject (Miss S.) and six with the other (Miss K.); the figures are, however, of interest, since they are in entire agreement with those of the preceding series.

Subject: Miss S.

	Contrast rate 76		Contrast rate 20	
	Av.	m.v.	Av.	m.v.
1st Reproduction of standard rate (50)	54·25	3·25	57·75	6·25
1st ,, ,, 'Contrast' rate	73·25	0·85	22·25	2·00
2nd ,, ,, standard rate (50)	56·75	1·60	49·62	5·08
2nd ,, ,, 'Contrast' rate	73·25	0·35	24·00	2·00
Subject: Miss K.				
1st Reproduction of standard rate (50)	47·67	1·10	49·00	0·67
1st ,, ,, 'Contrast' rate	72·33	0·43	20·17	0·23
2nd ,, ,, standard rate (50)	52·00	3·37	34·67	3·10
2nd ,, ,, 'Contrast' rate	65·00	2·67	19·17	1·10

The second reproduction of the 'contrast' rate was made with the view of discovering whether this would in turn be influenced, by way of Contrast or otherwise, by the immediately preceding reproduction of the standard. The figures obtained fail, however, to give us any reliable information on this point, since on two occasions (once with each subject) the second reproduction of the 'contrast' rate is affected in the manner we should expect if Contrast were at work, while on the two other occasions there is either no change or a change in the opposite direction.

The agreement of the results obtained from the four complete series here given seems to show that attentive listening for a short time to a sequence of auditory impressions following each other at a comparatively fast or slow rate, does exert a distinct contrast effect, when the attempt is made to reproduce a medium standard rate previously heard.

Owing to the simplicity of the experimental conditions of these observations, and the regularity of the results obtained, it seems likely that the procedure adopted by us, or some modification of it, will prove to be one of the easiest methods of obtaining and making an exact quantitative study of contrast effects.

While conducting the above observations we also made a number of experiments to see whether the direction of attention to the 'contrast' object was necessary for the production of the contrast effect. For this purpose the metronome was allowed to beat at the fast or slow 'contrast' rate for a considerable period (from one-half to three-quarters of an hour), while the subject was engaged as operator in some other experiments, and a reproduction of the standard rate made at the end of this period was compared with one made before the metronone was started. In these observations we made use of the recognition, as well as of the reproduction, method; but by neither method were any positive results obtained, the difference between the figures obtained before and after the 'contrast' stimulation being very small in all cases. A variation of the experiment produced a similar result. The metronome was taken home by one of us and made to beat at the standard rate for an hour or more each morning, in the room in which he was working. On the same afternoon he endeavoured to reproduce the standard. After this had been done on 20 days, the metronome was made to beat at the rate of 75 per min. for one hour in the morning of each of six days; and in the afternoon of each day an attempt to reproduce the standard was made, as before. It was intended to make a similar number of reproductions after hearing the metronome beating at 152 per min., but unfortunately the experiment had to be broken off when observations on two days only had been made. The average reproduction for the first 20 days was 51·07 (m.v. 2·42); that for the next six days (metronome 75 in morning) 52·72 (m.v. 2·09); that for the last two days 54·50 (m.v. 3·13). Of course it is just possible that the results of the two days with the metronome beating at 152 may represent a genuine contrast effect, and that the drop from 100 to 75 beats a min. was not sufficient to produce a degree of contrast measurable by our procedure; but in view of the high mean variation of the records made on the last two days this seems rather unlikely.

So far as they go, then, our experiments seem to show that, if attention is not paid to the contrast-inducing object or stimulus, no appreciable contrast effect is exerted by it.

(vi) *Judgment of rate of seen motion.*

Our investigation of contrast in the appreciation of the rapidity of motion was made towards the end of the period at our disposal for experimentation. We had, consequently, little time for the elaboration of apparatus specially suited for the observations we intended to make; we therefore made use of a piece of apparatus already possessed by the laboratory, which, though intended for another purpose, sufficed for a preliminary investigation. This was the Dotting Apparatus designed for testing mental fatigue, and described in a previous number of This *Journal*[1]. By means of this apparatus a long tape marked with an irregular row of dots at intervals of 5 mm. could be made to pass across an aperture at a speed which could, within certain limits, be controlled by adjusting "the friction of a pair of revolving blunt brass points, pressed by the centrifugal force of a pair of weights against the rim of a metal disc around which they revolve." Coarse adjustment was made by varying the weights used. The machine was provided with three different pairs of weights, giving three distinctly different rates of motion. The smaller weights, since they lessened the amount of centrifugal force, gave, of course, the quicker speed. For the purpose of our observations the medium weights were used for the standard rate of motion, while the smaller and the larger weights served to give a fast and slow 'contrast' motion respectively. Fine adjustment was made by varying "the pressure of a spring counteracting the pressure of the weights and regulated by a screw with a graded head"; the figures given in the following tables refer to the scale on this graded head. The higher the figure, the greater the pressure of the spring, the more the pressure of the weights is counteracted, and the faster, therefore, the rate of motion obtained. For our purposes the spool on which the tape is wound was fixed to a table in a different room from that in which the motor was placed; the tape passing from the spool to the motor along a specially constructed bridge. The subject sat in the room in which the spool was fixed, and watched the tape disappearing under the edge of a sheet of millboard. The operator controlled the motor from the other room, his movements being thus hidden from the subject. The friction regulator of the machine gives out a fairly loud note, which varies in pitch according to the rate of motion. As this was found to be audible even in the adjoining room, and would thus have informed the subject of the operator's manipulation of the machine, a large water motor, situated in the subject's room, was made to work slowly during the experiments, the noise produced by this rendering inaudible that from the machine. The subject, being thoroughly used to the noise of the motor, was, so far as he was aware, in no way disturbed by it.

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[1] Cyril Burt, "Experimental Tests of General Intelligence," This *Journal*, III. 153.

After a number of preliminary experiments, we adopted the following procedure:—The standard series was composed of ten reproductions of the standard rate (7[1]-medium weights), which was presented to the subject for 10 secs. before each attempt. Five reproductions of the standard were then made, without the standard itself being presented. The 'contrast' series consisted of ten reproductions of one of the two 'contrast' rates (7-small weights or 7-large weights, on alternate days), which also was presented to the subject for 10 secs. before each attempt. The 'comparison' series was then made, consisting of five attempts to reproduce the standard rate. The operator, of course, regulated the rate of motion of the tape, very slowly increasing it until told to stop by the subject. The following are the results of the only complete set of observations performed, consisting of six experiments on six successive days:—

Subject: W. McD.

Standard rate	Before 'contrast series'	After quick 'contrast series'	Before 'contrast series'	After slow 'contrast series'
Av.	6·72	7·10	5·68	5·16
m.v.	·38	·36	·54	·44
Av.	6·98	8·14	6·34	5·08
m.v.	·48	·60	·40	·42
Av.	6·94	7·86	5·68	3·94
m.v.	·76	·68	·54	·48

It will be seen that, in spite of some obvious imperfections in the experimental conditions of our observations, and in spite of the small number of observations recorded, the figures obtained afford strong evidence of the existence of a considerable contrast effect. It therefore seems likely that a longer investigation performed under more suitable conditions would not fail to give interesting results.

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[1] This figure indicates the position of the pointer on the scale of the head of the screw which regulates the rate of motion of the tape.

III. CONCLUSION.

We do not think that the experimental data collected by ourselves and other investigators are yet sufficient to render profitable a reconsideration of the various theories that have been advanced concerning the nature of the phenomena we have brought together under the head of Psychological Contrast. The similarity of the results obtained by us in experiments involving several apparently different mental processes seems, so far as it goes, to afford some evidence in favour of the view that there is one general Law underlying all contrast effects of central origin, such as we have postulated in the first section of this paper. On the other hand, the fact, experienced both by ourselves and by other workers, that contrast effects are by no means always manifested under conditions which we should expect *a priori* to be favourable to them and which seem quite comparable to those under which they do undoubtedly occur in slightly different mental operations, suggests that the so-called contrast effects obtained in different ways are largely independent of each other and probably due to a different cause in each case, the apparent similarity of the effects being mainly accidental and in no way due to the operation of a general Law of Contrast. The facts before us at present are, however, quite insufficient to *establish* either of these views. Thus it is possible that, where at present we see only similarities between the contrast effects found in different mental operations, more accurate experimentation may show that there really exist important differences between the effects obtained in different cases, thus rendering more probable the view that the resemblance between them is merely superficial. It is, however, also possible that future investigation may show that, in our apparent negative instances, we have overlooked the absence of some condition which is essential to the operation of the Law of Contrast.

It seems to us that careful quantitative experimentation on two problems indicated by our own work may eventually throw considerable light on this question. These problems are:—

(1) The relation of the magnitude of the contrast effect to that of the difference between the 'contrast' and the standard objects. That such a relation does exist is indicated by three of our own experiments,

viz. those on lifting weights, comparing colours, and reproducing lines upon backgrounds of different size. In these cases the contrast effect seemed to be the more marked, the greater the difference between the 'contrast' object and the standard object. It seems likely, however, that this rule can only hold up to a certain point, and that beyond this point the Contrast must decrease with increase of the difference between the 'contrast' and the standard object. To discover at what point this change occurs, whether the change itself is abrupt or gradual, and what is the exact relationship between the intensity of contrast effect and the difference between the 'contrast' and the standard objects on either side of the point of change—these are problems which it would seem possible to solve by the methods described above, but which will require much more prolonged experimentation than has hitherto been attempted.

(2) The relation between the strength of the contrast effect and the length of the interval between the presentation of the contrast object and that of the comparison object. That the contrast effect persists for a comparatively long period is shown by our experiments on Psychological Colour Contrast, where, in order to exclude the effect of Physiological Contrast, we were compelled to allow a considerable time to elapse between the contrast and the comparison series. In one of our series of observations on the estimation of the rate of succession of auditory impressions, the results seemed to indicate that the contrast effect, though still existing, had declined appreciably in half an hour. Further investigation on this point would seem not difficult and might produce some interesting results.

If future experiments show that in these two respects the contrast effects obtained in different cases are strictly comparable, this would seem to be strong evidence in favour of the view that all the several contrast effects that we have studied and roughly measured (and probably many others also) are to be explained by some one principle, as we have assumed in our introductory discussion.

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PERSEVERATION.

BY W. LANKES.

(*From the Psychological Laboratory, University College, University of London.*)

INTRODUCTORY NOTE.

BY C. SPEARMAN.

THE following paper was sent to me from India by Dr Lankes for revision, supplementation, and eventual publication. But hardly had the manuscript been posted when its author—a man to a rare degree admired for his scientific attainments and loved for his personal qualities—suddenly succumbed to an attack of dysentery. Under these circumstances it seemed best to leave the MS. untouched, save for a few verbal changes and this brief note explaining its origin.

A very extensive research by Dr E. Webb had given birth to a momentous problem. He had found that human character, and especially the differences between individuals, has one paramount factor, namely, the degree in which conduct is controlled by deliberate resolution rather than by present impulse. An explanation at once suggests itself. As the more deliberate motive is usually supposed to be merely one of older date, the persons who most obey deliberate motives may well be those on whom experiences leave the most persistent impressions.

If this is true, the facts observed by Webb would fall well enough under the general theory of O. Gross, Wiersma, Heymans, and others, that one of the most fundamental differences between individuals lies in the intensity of the 'after' or 'secondary' function exercised by ideas after lapsing from distinct consciousness. Akin also is the well-known 'perseveration' theory of G. E. Müller and his school; according to this, one of the chief individual differences is the varying degree in which their ideas after disappearing from consciousness continue to 'perseverate' unconsciously.

This, then, is the problem undertaken by Dr Lankes, to ascertain whether Webb's phenomenon is only a special instance of some law of persistency of impressions in general, or whether it is a law peculiar to the operations of the will. He arrives at a definite solution in the paragraphs that follow.

The present paper gives a description of a fairly large set of experiments on Perseveration, together with a short summary of results. For a review of the literature and previous work on this subject, the reader is referred to E. Wiersma, "Die Secundärfunktion bei Psychosen," *J. f. Psychol. u. Neurol.*, 1906, Vol. VIII. Nos. 1 and 2.

In most investigations hitherto the number of subjects, and often the range covered by the experiments, has been rather small; it seemed desirable to extend in both directions and obtain a broader basis of fact. This the present writer tried to do. The object of the investigation was to contribute something towards elucidating two important questions: (1) Is Perseveration a general factor,—comparable with General Ability, influencing the entire range of mental activity, and therefore of prime importance for a correct conception of the constitution and operation of the human mind? (2) How is Perseveration, as a peculiarity of the cognitive side of mental life, related to the 'Perseveration'-qualities of character, *i.e.* perseverance or persistency of will? The latter question became of special interest owing to the work of E. Webb, "An attempt at an exact study of character," read (1-2) at a meeting of the British Psychological Society, and to be published as a monograph supplement to this *Journal.*

The entire research was started under the direction of Professor Spearman, and carried out with his continual help and advice. To him I wish to express my sincerest gratitude for his unwearied kindness and interest. The first set of experiments was carried out in the Psychological Laboratory at University College, London, May to November, 1913, with 15 subjects, all members of the staff or students of psychology; the second during the summer vacations of the same year, partly at University College, partly at Manresa House, Roehampton, with 18 students (mostly undergraduates) of the latter institution. These two sets were preliminary only. The final set of experiments occupied March and April, 1914, the marking and evaluating of the results taking up several months more. Through the kind mediation of Mr E. Webb, 47 students of his college, Islington Day Training College, London, were obtained as subjects. The tests were often rather wearisome and tedious, especially during the long preliminary trials. All, however, worked with admirable interest, sacrificing many a recreation hour to the experiments. I beg to express my heartfelt thanks to them, and to Mr E. Webb and his assistant, Mr G. Tyre, for most valuable help; and to all my subjects, both at University College and at Manresa House, for their never-flagging readiness and attention in performing the tests.

In order adequately to treat Perseveration, a course of experiments and observations must cover the following phenomena:

(1) The persistent after-effect of a sensory experience, *i.e.* the continuance, more or less prolonged, of the physiological and psychical impression beyond the real duration of the external stimulation.

(2) The spontaneous recurrence to consciousness of an experience, without a fresh corresponding stimulation, after it has for a time been out of the mind.

(3) The continuance, subconscious, or even completely unconscious and purely physiological, of the effect of a past experience, as presupposed by (2), that is to say, by the very possibility of remembering and recalling what once was, but at the time is not, in the mind.

In the last two forms especially care must be taken not to confuse Perseveration with mere excellence of memory.

All these forms of perseveration I endeavoured to test. Forms (1) and (2) allow of direct approach by observation and experiment. The former would seem to be tested by the Colour Disc experiment (page 397 ff.) and by Interrogatory, question 17*a* (page 414), the latter by Associative Reaction, and by Interrogatory, questions 1, 2, 3, 8, 17*b* (pp. 413, 414). Form (3) as distinct from power of memory, can probably be tested indirectly only, by ascertaining the degree of hindrance which the perseverating effect of a past mental activity causes to a new one of the same kind. This has been attempted by Interrogatory, questions 6, 7, 10–14, 16 (p. 414), by Letter-Writing (p. 391 ff.), Cancellation (p. 394 ff.), Drawings (p. 404 ff.), Narratives (p. 406 ff.), and Essays (p. 412). This inhibition of a succeeding by the effect of a preceding experience must also be expected to reveal itself in the relative slowness of the perseverator: Natural Rate of Tapping (p. 390), Associative Reaction Time (p. 409 ff.).

I shall now proceed to describe in detail the experimental methods used.

I. NATURAL RATE OF TAPPING.

The instruction given to the subjects, all 47 working together, was the following: "I ask you to sit quite at your ease, to shut your eyes in order not to be disturbed by your neighbours, and just slightly to move your finger as in tapping. Do not really tap on the table, as that would cause noise and disturb you. The essential and important point in the experiment is the rate or speed of the movement: your task is simply to move the finger each one at his own rate, just as he feels it natural to himself at the time. So the point is not to move or tap fast, or slow, but just as it comes naturally and spontaneously to each one. And I ask you, to count your movements, the number of taps, yourselves. I give two signals: First 'ready,' then 'now'; start at once at the 'now'; do not forget to count the taps, and stop at once, when I say 'stop.' And then you will write down the number of movements made, on the paper I have given you."

The experiment was done twice, on two successive Fridays. Each day it consisted of three trials, each lasting 10 seconds, and succeeding one another after pauses of about one minute. The individual variations from trial to trial on the same day are small: with 34 subjects on the first day, and with 41 on the second, the difference between the greatest and the smallest number of movements made during the 10 seconds is not more than two. The greatest variations by far are represented by the differences: 18–12, 28–18, 31–24 on the first day, and 18–9, 32–19, 62–56 on the second. Three subjects on the first, and nine on the second day returned the same number in every trial. Correlating the average of the three trials of the first with the average of those of the second day, we find $r = \cdot 85$[1]. The correlation of this with the other experiments is based upon the mean value of the two averages. The number of movements or taps per 10 seconds of the 1st (*i.e.*, slowest), 6th, etc., subject is represented by:

Rank of subject	1	6	12	18	24	30	36	42	47
Number of taps per 10 sec.	8	11	13	14	16	18	23	26	57

As some persons, *e.g.*, piano-players, are far more accustomed to (rapid) finger- or hand-movements than others, it might prove advantageous to try elbow-, or foot-, or knee-movements, or nodding instead of, or alternately with, tapping. In this way irrelevant, disturbing variations could possibly be eliminated, and a purer 'natural rate of movement' be obtained. The experiment is easily performed and easily evaluated, of fairly high reliability, and gives a positive, though small correlation with every other one of the set, showing that the perseverator on the whole tends to be slower than the non-perseverator.

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[1] In the entire set of experiments the correlating was done according to Spearman's 'Foot-Rule,' and the R-value transcribed into the corresponding r according to Table 57 in Thorndike's *Introduction to the Theory of Mental and Social Measurements*, 2nd ed. (1913), 227.

II. LETTER-WRITING.

In the preceding experiment on the 'natural rate of movement,' the number of taps made per 10 seconds could itself be regarded as an inverse measure of the degree of Perseveration, the latter being the greater, the slower the movement, the fewer taps in the given time. The Letter-Writing test afforded no such easy or direct index. All that could be expected was that the strength of Perseveration would manifest itself indirectly, in the amount of disturbance caused by changing the order in which the letters were to be written, the Perseverator being more liable to suffer under the shock of the change than the Non-Perseverator. As the experiment was intended to serve as a motor test, not as one of ideation, the same set of six letters was used in the four trials of each day. On the first, second, and third trial they were to be written in their alphabetical order, on the fourth in the reversed order. The four sets of letters used on the four days (always a week apart) were: (1) *tuvwxy*, (2) *hijklmn*, (3) *bcdefg*, (4) *nopqrs*. Thus there could be no ideational difficulty, *i.e.*, in remembering and mentally holding fast the required sequence of the individual letters, neither in the forward, nor in the reversed order; besides, the subjects were asked to write out the respective set as a heading to the first, and again (reversed) to the fourth part of their task. They were also enjoined to write on continuously, as if all were but one word, not breaking off and starting anew after each letter or after the sixth letter of the set.

The four trials of each day succeeded one another, separated only by pauses of a minute, when the subjects were again asked to try their best and write as much as they possibly could during the following 30 seconds—the time that each trial lasted. The efficiency of the 47 subjects (all did the experiment together) is represented by the number of letters correctly written in 30 seconds; every mistake in the order of sequence was counted as −1, *i.e.*, subtracting one letter. There is, on the whole, an improvement both from trial to trial and from week to week. The following figures illustrate this. (Only 41 subjects were present, and are ranked, for every trial; the others are only represented in the mean by their average.)

Rank of subject			1	6	11	16	21	26	31	36	41
Efficiency	1st day	1st trial	70	60	58	54	54	48	48	40	30
		2nd „	72	66	60	56	54	54	51	48	38
		3rd „	73	69	66	60	58	56	54	52	42
		4th „	66	54	51	49	46	43	40	36	26
	4th day	1st trial	90	82	76	72	70	66	60	59	44
		2nd „	93	87	81	78	75	72	69	66	60
		3rd „	90	84	84	81	79	78	72	66	59
		4th „	87	75	72	66	66	62	58	55	31

The average efficiency in the third trial, taking the four days together, and the corresponding average efficiency in the fourth, is shown in the next table.

Rank of subject	1	6	12	18	24	30	36	42	47
Average efficiency in 3rd trial	83	79	75	72	70	68	65	62	57
Average efficiency in 4th trial	74	64	61	58	55	53	50	45	36

The correlation of the efficiency in the first trial with that in the second, and of the second with that in the third, is $r = \cdot 75$ and ·82 for the first day, ·87 and ·84 for the second, ·74 and ·85 for the third, and ·71 and ·65 for the fourth day. So the reliability of the experiment, as long as the same set of letters is retained, is fairly high. But it sinks considerably on comparing different days, with different sets of letters. The correlation between the efficiencies in the first trials of the four successive days is $r = \cdot 62$, ·46, ·49 respectively; so the influence of the set of letters used, their greater or less difficulty, seems to be appreciable. The effect of the reversal of the order in which the six letters are to be written in the fourth trial, manifests itself also in a lowering of the correlation between the efficiency in the third and that in the fourth trial. This correlation is $r = \cdot 69$, ·61, ·29, ·46 for the four successive days.

The measure of the degree of disturbance caused by the reversal of the sequence of letters in the fourth trial, *i.e.*, the measure of the degree of Perseveration with each subject, was at first sought in the quotient

e_3/e_4, where e_3 stands for efficiency in the third trial, and e_4 for efficiency in the fourth. But the reliability of this quotient proved very low indeed ($r = \cdot 12$, ·24, ·11, comparing first and second, second and third, and third and fourth day). Besides, Me_3/Me_4 gave a very low correlation with other experiments (Rate of Tapping, Colour Discs). Several other factors were then tried: substituting the average efficiency in the three first trials taken together for e_3; or $(e_2 + e_3)/2e_4$; or $e_3/e_2 - e_4/e_3$; but none of these mended matters, except the one involving the greatest amount of arithmetic: $1/2\ (e_2/e_1 + e_3/e_2) - e_4/e_3$. This factor gave the reliability-measures $r = \cdot 16$, ·32, ·41. The result of the first day was ultimately rejected; and for correlating with the other experiments, the average of the above factor for the second, third, and fourth day alone was used. The following table illustrates the values thus obtained, the higher figures corresponding to the higher degree of disturbance under the influence of the change:

Rank of subject	1	6	12	18	24	30	36	42	47
Value of disturb.	·60	·41	·35	·31	·28	·25	·22	·19	·15

It might prove more advantageous to give a longer time, at least one minute, for each trial, not only 30 seconds. In the present case the shorter time was chosen in order to avoid the practice-effect's interfering with, and hiding, the Perseveration-effect in the fourth trial; but I have been led to think that in 30 seconds the Perseveration effect does not fully show itself. When experimenting with the somewhat similar Cancellation test, I often asked the subject whether, in the performance with changed letters, the old letters, those used before, had come to his mind again and hindered him, and very frequently the answer was: "Yes, towards the end," *i.e.*, 2–4 minutes after the change had been introduced. The reliability of the factor used as measure of the degree of the disturbance, and of perseveration (possibly also the quotient e_3/e_4), might be improved by using one and the same set of letters (perhaps 10 or 12, instead of 6, to prevent familiarity) throughout, on every day, not a fresh, different set each day. The amount of disturbance caused by the change would probably be increased by not simply reversing the order or sequence of letters, but changing it in any other way, or partially changing and partially retaining it, *e.g.*, employing *bcdefghij* in the first, second, and third trial, and *jihgdefcb* in the fourth; but thereby the test would seem to change its character considerably, to become more ideational, less purely motor, as the chief cause of the disturbance would now lie in the difficulty of mentally holding fast the order of sequence of the letters in the fourth trial, not of merely writing them in that order.

III. CANCELLATION.

In previous work with this test I had become aware of the very great influence of the difficulty, or the opposite, of the particular letter or group of letters selected for cancelling, on the efficiency displayed by the subject. There seems to be practically no transfer of the skill acquired in crossing one letter, or one group of letters, to the carrying out of the same task with a different letter or group. The following table may serve to illustrate this for single letters. The two subjects C and S are undergraduates, aged 19, B is a Doctor of Philosophy, aged 36. The experiment was performed on four days, distributed over a fortnight. Each day there were four successive trials with one letter, followed by one trial with a different letter; then, after an interval of about 25 minutes occupied with other work, there came a second trial with the second letter, and a last trial with the first: the intervals for rest between successive performances lasted one minute (excepting that between the fifth and the sixth performance).

	I day							II day						
Subj. Letter used:	*C*				*F*		*C*	*L*				*R*		*L*
B	1·11	1·17	1·11	1·06	·98	·92	1·17	·94	·89	1·01	1·22	·91	1·15	1·12
C	1·54	1·82	1·85	2·00	1·29	1·10	2·08	1·02	1·15	1·38	1·29	·82	·75	1·64
S	1·52	1·98	1·78	2·07	1·08	1·27	2·13	1·29	1·27	1·15	1·39	1·17	1·30	1·55

	III day							IV day						
Subj. Letter used:	*S*				*K*		*S*	*N*				*D*		*N*
B	1·32	1·23	1·54	1·56	·95	·93	1·59	1·08	·99	1·25	1·18	·77	1·20	1·20
C	1·96	2·00	2·16	2·56	1·02	·96	2·86	1·10	1·10	1·14	·93	1·67	2·04	1·20
S	2·04	2·37	2·38	2·56	·92	·90	2·63	1·14	1·36	1·37	1·31	1·33	1·67	1·25

The efficiency represented by the figures changes pretty regularly together with the change of letters. In spite of the 'set' established by working with the same letter four times in succession, the change may mean a rise of efficiency, if the new letter is easier than the old one was; or it may mean a very big drop, if the new letter is much more difficult; cf. the rise, with the two subjects, C and S, with *D* after *N*, and their fall with *K* after *S*. Quite similar is the result when working with four letters at once, instead of with a single one. The groups *ILTV* and *COSX*, for instance, are found easy and give a high efficiency no matter what goes before; *AERW* and *GKXZ* are hard and import a fall. Besides, one and the same letter behaves differently when it is to be cancelled alone, and when together with three other letters. In the latter case very much depends, it seems, on whether the four letters can easily be combined into some sort of unit, as *COSX* into '*cos X*.'

For computing the efficiency index, the formula given by Whipple, *Manual of Mental and Physical Tests*, 1910, p. 260, was used, *i.e.* efficiency $= E = A/T$; where T stands for the time taken, 100 seconds being regarded as unit, and A for the index of accuracy, $= (c - w)/(c + o)$, in which $c =$ number of letters actually crossed, $w =$ number of letters wrongly crossed, and $o =$ number of letters omitted though they should have been crossed.

The prints employed both in the experiments spoken of above and in those to be discussed presently, were of two different sets of equal length, each consisting of 20 lines of capitals and containing 80 letters to be crossed (every one of the letters intended for cancellation occurring 20 times) in a total of about 600. The 47 subjects performed the experiment together, the time being determined by the fastest subject: it was part of the instruction that he who finished first should call out 'stop,' on which all had to stop immediately. This method of timing was used on the third and fourth days, and was found to work best; on the first day I had allowed two minutes, but noticed that several finished before that time was over; and on the second day (the letters being the same as on the first day, and therefore a considerable amount of improvement to be expected) I fixed upon 90 seconds, which again proved too long for a few of the fastest subjects. In order to establish a strong 'set' before introducing the change, only one group of four letters (*COSX*) was crossed on the first day in three successive performances (one minute rest between), and again on the second day. In the fourth trial of the second day, when thus a high efficiency with this group of letters had been reached, the change was brought in: *HKPY* were to be cancelled now. The same was done with the letters *ILTV* and *GNRZ* on the third and fourth days. In either case the more difficult group, *HKPY* and *GNRZ*, was selected for the change, because in preceding experiments it had shown itself clearly, that even strong perseverators are not at all thrown back, but, on the contrary, considerably gain in efficiency, by the change, if the new set of letters happens to be easier than the old one was. One of the two texts or prints was used throughout the first and second days, and the other throughout the third and fourth.

Of the 47 subjects 39 were present in all trials and could be ranked for determining the reliability of the test; the others are only represented in the mean used for correlating with the other experiments. The following table may serve to give an idea of the values (efficiency $= E$) obtained in the successive performances.

Rank of subject		1	5	10	15	20	25	30	35	39
1st day (*COSX*)	E_1	·80	·63	·58	·54	·51	·47	·39	·39	·28
	E_2	·80	·74	·67	·66	·57	·55	·52	·47	·42
	E_3	·81	·78	·72	·67	·63	·62	·57	·51	·42
2nd day (*COSX*)	E_1	1·04	·90	·86	·80	·73	·68	·66	·58	·39
	E_2	1·07	·93	·88	·82	·79	·73	·67	·62	·44
	E_3	1·06	·97	·94	·90	·84	·80	·77	·66	·54
(*HKPY*)	E_4	·61	·56	·52	·49	·47	·43	·39	·34	·30
3rd day (*ILTV*)	E_1	·79	·74	·64	·57	·55	·52	·49	·43	·33
	E_2	·81	·73	·68	·63	·59	·55	·54	·49	·37
	E_3	·87	·79	·72	·67	·63	·59	·57	·49	·37
4th day (*ILTV*)	E_1	·89	·84	·79	·71	·66	·62	·60	·53	·43
	E_2	·96	·88	·83	·79	·73	·68	·65	·61	·50
	E_3	1·08	·95	·82	·80	·74	·73	·69	·62	·50
(*GNRZ*)	E_4	·61	·56	·50	·47	·45	·41	·37	·28	·20

The correlations of successive performances are: $r = ·85$, ·86, ·84, ·91, ·92, ·41 (change of letters!); ·44 (again different letters), ·84, ·88, ·85, ·91, ·89, ·53 (change of letters!). E_1 of the first day correlates with the E_4 of the second only to the extent of $r = ·29$, owing, as it would seem, to both the difference of letters and the different amount of shock experienced by the subjects on the introduction of the change. That much of the disturbance is due to the peculiar difficulty of the new letters, rather than to the resistance of Perseveration, is evidenced by the fact that the corresponding correlation between E_1 of the third day and the E_4 of the fourth is considerably higher, $r = ·67$. The same point is brought out by the low correlations $r\,(E_3^{IV}, E_3^{II}) = ·67$, $r\,(E_4^{IV}, E_4^{II}) = ·58$, $r\,(E_1^{III}, E_1^{I}) = ·61$. ($E_3^{IV}$ = efficiency in the third trial of the fourth day; E_3^{II} = efficiency in the third trial of the second day, etc.)

As a measure of the degree of disturbance caused by the change, and consequently of perseveration, the quotient E_3/E_4 was first tried. The reliability of this factor is $r = ·37$. But the corresponding quotient of the averages of the E_3's, and E_4's, of the second and the fourth day M_{E_3}/M_{E_4}, gives only the correlations $r = ·27$ with the Colour experiment, ·23 with Letter-Writing, and 0 with Natural Rate of Tapping. Consequently here also the more elaborate formula (1),—

$$1/2\,(E_2/E_1 + E_3/E_2) - E_4/E_3$$

was chosen. This too yields the reliability $r = \cdot 37$. The correlation of the mean of the two available values of it with the other experiments is shown in the general table (p. 416). The following exhibits some representative figures of this mean.

Rank of subject	1	6	12	18	24	30	36	42	47
Value of disturbance	·76	·67	·60	·57	·51	·50	·44	·38	·17

To judge from expressions used by the subjects, and also from their behaviour under the experiment and the variations of efficiency from letter to letter, and from one group of letters to the other, the Cancellation test would seem to be of a very mixed type: motor, perceptional, and ideational, the latter element greatly prevailing when four, or more, letters are to be cancelled, whilst the motor character comes out more in the crossing of one letter only.

IV. COLOUR DISCS.

E. Wiersma[1] has long shown that in observing the red and green sectors of a disc rotating at an increasing rate of speed, Perseverators lost the two colours, and commenced to see only grey, much sooner, *i.e.*, at a lower number of rotations per second, than Non-Perseverators. The impression made by the red has already died away with the Non-Perseverator, and not yet died away with the Perseverator, when the green impinges upon the same nervous element or group of elements; hence the fusion-grey comes at lower rotation speed with the latter than with the former. But in experimenting with a low rotation speed at first, and only gradually ascending to a higher one, the subject of course knows from the outset which colours are actually on the disc; they are in his mind, and he may imagine, or think and judge them still visible, read and interpret them into the more or less vague and indefinite 'grey' gradually arising. In other words, it becomes doubtful, whether this Perseveration lies in the peripheral or the central elements, is sensational, or perceptual, or ideational.

To eliminate this drawback, and, if possible, to gain some insight into the nature of the process, a different way of procedure was here adopted. A number of different discs were used, five in the preliminary work—red-green, pale green-crimson, blue-yellow, slate-orange, black-white—and four (omitting the first) in the last set of experiments with my 47 subjects at Islington College. The colours, and the size of the sectors were so chosen that at a rotation speed of about 20 rotations per second they all gave pretty much one and the same grey; only with the blue-yellow disc there remained, for several subjects, a tint of the blue throughout, somewhat, though but slightly, modifying, 'spoiling' the pure grey. Besides, I began experimenting with a high rotation speed, in the final set of experiments—25 rotations per second. It became apparent in the course of the preliminary work, that a uniform, grey background was necessary, and that not the whole disc should be exposed during the actual trial, but only a narrow slit. When the whole disc was exposed, it occurred that the subject, on moving the eye, got a sudden, quite distinct, though very short, perception of the colours really on the disc even at a speed at which normally, with eyes fixed and not following the motion of the disc, he could see nothing but grey. Therefore the screen of grey was fastened in front of the disc, so that the latter was visible only through a slit 3 cm. broad and 11 cm. long (the diameter of the discs was 13 cm.). This slit was itself covered by a movable shutter, a photographic plate quite black, and overlaid with a slip of black paper on the upper end, becoming more and more transparent toward the lower; the shutter could be moved up and down in a guiding frame fastened to the back of the grey screen, and was always kept down, hiding the disc from view, until the proper rotation speed was reached and the experiment actually began. So the subjects could not possibly know from the outset which were the colours actually on the disc.

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[1] *Loc. cit.*

It became obvious at an early stage that, if the observers were only told to write down the name of the colour, or colours, which they saw on the rotating disc, there was no guarantee that any two subjects, or one and the same subject on different occasions, even during the same sitting, meant by the same word or name really the same sensation. To get rid of this source of variability and error, 18 small patches of coloured papers, each patch 1 cm. broad and 2 cm. long, were gummed on a strip of grey paper and this was stuck on the screen just underneath the slit through which the rotating disc could be seen. The patches contained the precise colours really on the discs, besides six others lying between. They were arranged in three horizontal rows of six running parallel to the slit, so that they could all easily be observed together with the colour, or colours, appearing in the slit. Every patch had, in big, clear figures, its number (from 1 to 18) written just above it.

The instruction given to the subjects, type-written, and carefully explained on the first day, and recalled to mind on the following days, was this:

1. After 'now' carefully observe the colour in the slit (on either side of the little brass screw in the centre, of course not minding the colour of the brass), as it appears to you during the exposure (six seconds). Observe merely what you actually see; do not guess or try to remember, or to infer, the real colour of the disc. Attend to whether you see one colour only, or two, or three alternately, and make out which of the colours on the paper underneath the exposure slit each one of them is most like. Do not mind one or two occasional and quite irregular flashes of colour, but only the colour or colours which remain, or keep recurring constantly, during the whole time of the exposure.

2. After 'line 12,' 'line 11,' etc. [each subject was given an octavo leaf on which he noted name and date, and which contained 12 lines marked on the left margin 12, 11, 10,...2, 1] write the number of the colour or colours which you have seen on the line marked 12, 11, etc.; for instance, 'No. 2' would mean: "I saw colour No. 2, one steady grey, of all the colours on the paper most like the one marked 2, though perhaps not precisely the same."

'No. 2 + ?' *i.e.*, "I saw one colour only, most like No. 2, but there was continually flickering across it something else, the colour of which I could not make out; just No. 2 flickering."

'No. 9 + 15,' *i.e.*, "I saw two distinct colours, most like No. 9 and No. 15 respectively."

'No. 10–11,' 'between 10 and 11,' *i.e.*, "I saw only one colour, not likely any single one on the slip, but rather a mixture, lying between Nos. 10 and 11."

Whilst the subjects recorded their colours, the experimenter set the apparatus for the next degree of speed, by sliding the cord, which ran over the axle bearing the disc and a cone of pulleys, into the corresponding groove of the latter. The axle carrying the disc was 1 cm. in diameter, the pulleys (bottom of groove) 25, 23, 21, 19,...7, 5, 3 cm. So by turning the handle of the pulley throughout at the constant rate of one rotation per second, which, after some practice, could be performed fairly well, the required speed was easily and securely obtained. Before being used in the experiment, the apparatus, and the proficiency of the experimenter in turning at a constant rate, were carefully tried and tested with the speedometer along the entire range of the pulley. The apparatus works without any noise whatever. The room in which the experiments were carried out, was only lighted up with one electric 'wirum' lamp of 32 candle power (200 volts) standing on the same table as the apparatus, the bulb right opposite to, and on a level with, the exposure slit, at a distance of 3½ ft. from the latter. Daylight was all but completely excluded by heavy dark blankets covering up the two windows. The subjects, 10–12 at a time, stood, or sat on high stools so as to have their eyes as nearly as possible on a level with the exposure slit, in two arched rows just behind and to either side of the lamp, taking care not to have the light falling into their eyes. They were, of course, repeatedly asked not to speak during the experiment and not to look at their neighbours' papers, in order not to be influenced by one another, but carefully and conscientiously to record each one merely what he saw himself. And they all worked throughout with exceedingly great attention and interest. Besides, they knew that they had no occasion to wonder at finding out, in the pauses when one disc was exchanged for another, that they had seen quite different colours; for previously to the colour-disc experiment all the 47 subjects had individually been tested for defects in colour vision, according to Prof. Stilling's *Pseudo-Isochromatische Tafeln*, 1913, 14th edition.

There were on the whole four sittings for each group of ten or twelve, and the same four discs were gone through in the same order of sequence every time. Each sitting took about 40 minutes. In the first and second trials the experiment began with rotation speed 25 per second, and descended through all the 12 steps, 23, 21, 19,...5, 3 rotations per second. In this process the subjects had no foreknowledge whatever; it was all 'grey' at first. In the third and fourth trials the sequence of steps was reversed. Again, all the 12 steps were gone through, but beginning with the lowest speed, or rather with none at all; for each disc was first exposed standing still, and the subjects were asked to write down the numbers of the colours as they saw them then; after that began the gradual ascent.

It would be a special study fully to describe and interpret the data contained in the 700 and odd papers returned by the 47 subjects. In very many cases a strong, distinct, quite unmistakable yellow is recorded, when actually there was only pure black and white on the disc; and this yellow might appear in place of the black, or of the white, or of both, so that sometimes the only colour seen was yellow. With the slate-orange disc (50 degrees of orange, all the rest slate, nearly akin to greenish grey, altogether unimpressive and unobtrusive), very often, almost regularly, a bright, well-marked green appeared together with a red, sometimes before any red or orange; the two colours really on the disc were only made out at a relatively low rotation speed. In a set of experiments with the colour discs on Perseveration this slate-orange disc might well be used in place of red-green and of yellow-blue. It correlates $r = \cdot 40$ with Rate of Tapping, $r = \cdot 29$ with Letter-Writing, ·12 with Cancellation, ·50 with Drawings; the corresponding correlations for the other three discs are: for light green-crimson: ·23, ·26, ·40, ·38; for blue-yellow: ·26, ·04, ·23, ·07; for black-white: ·05, ·23, ·09, ·24; for the average of the three 'coloured' discs (exclusive of black-white) taken together: ·39, ·27, ·40, ·50. These were the results of the 'descending' method, *i.e.*, proceeding from highest to lowest rotation speed; the 'ascending' method gave on the whole somewhat lower correlations.

In marking and evaluating the data returned by the subjects the formula for determining the average limit according to the method of Constant Stimuli, or Right and Wrong cases, not admitting undecided answers, was used. The formula is $L =$ maximal variable $- i\ (Nr/m - 1/2)$, where $i =$ interval between the variables, in our case $= 2$ rotations per second; $Nr =$ number of right answers returned by the subject, and $m =$ number of repetitions of the same variable, in our case $= 1$ for each disc in each sitting[1]. As the maximal variable was represented by the lowest number of rotations per second, the first minus in the formula had to be changed into a plus; so my real formula was: $L = 3 + 2$ (number of right answers of subject $- 1/2$). To determine the number of right answers, a detailed scheme was drawn up showing the value of every possible No. of colour on the comparison slip returned by the subject for each one of the colours actually on the disc. When the two colours were both correctly marked, it counted 1, *i.e.*, one correct answer; any single one of the two colours on the disc correctly given counted ·5; whenever not the true colour or hue, ~~nor~~ not one very near, and practically equivalent with it was given, it was marked ·4 or ·3, according to the degree of approximation to the true answer. Decidedly different hues counted 0. Thus the method of marking had nothing rough or arbitrary, and followed as closely as might be the variations of the actual data.

The following figures, omitting decimals, are samples of the values, *i.e.*, rotations per second, actually obtained (average of two performances).

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[1] See formula (*b*), this *Journal*, 1908, II. 235.

Rank of subject	1	6	12	18	24	30	36	42	47
Red-green, descending	8	12	15	16	16	17	18	20	25
„ „ ascending	11	14	15	15	16	17	18	21	23
Blue-yellow, descending ...	10	12	14	15	15	16	17	19	22
„ „ ascending	12	14	16	16	17	18	19	20	23
Slate-orange, descending...	6	10	12	14	14	15	16	17	20
„ „ ascending	8	11	13	13	15	15	16	17	18
Black-white, descending ...	12	14	15	16	17	17	19	21	22
„ „ ascending	13	15	16	17	18	19	19	21	23
Average of descending	11	12	14	15	16	16	17	18	20
The 3 colours ascending ...	11	14	15	16	16	17	18	18	21

The reliability of the experiment, correlating the two trials with each disc, is for:

Red-green, descending, and including colour-weak subjects, $r = \cdot 65$; excluding colour-weak subjects, $r = \cdot 67$;
„ ascending, „ „ „ ·80; „ „ „ ·71;
Blue-yellow, descending, „ „ „ ·68; „ „ „ ·71;
„ ascending, „ „ „ ·68; „ „ „ ·73;
Slate-orange, descending, „ „ „ ·73; „ „ „ ·61;
„ ascending, „ „ „ ·65; „ „ „ ·75;
Black-white, descending, „ „ „ ·78; „ „ „ ·76;
„ ascending, „ „ „ ·76; „ „ „ ·81.

The correlation of the average of the two trials with each disc in the ascending method compared with the corresponding value in the descending method, is strikingly lower throughout. It is for:

Red-green, including all 47 subjects, $r = \cdot 31$; excluding the 10 colour-weak, $r = \cdot 31$;
Blue-yellow, „ „ ·37; „ „ „ ·37;
Slate-orange, „ „ ·55; „ „ „ ·41;
Black-white, „ „ ·34; „ „ „ ·23;

Average of all 4 discs „ „ ·29; „ „ „ ·26;
Average of the 3 coloured discs „ „ ·34.

It may be of interest to give the tables showing the correlations (r), of the various colours among themselves, according to the two ways of experimenting, *i.e.*, beginning with the highest, or with the lowest speed, including, or excluding, or considering alone, the ten colour-weak subjects.

(i) Beginning with highest rotation-speed, and including all subjects:

	Red-green	Blue-yellow	Slate-orange	Black-white
Red-green	—	·19	·53	·02
Blue-yellow	·19	—	·47	·27
Slate-orange	·53	·47	—	·07
Black-white	·02	·27	·07	—

(ii) Beginning with highest rotation-speed, but excluding colour-weak subjects:

	Red-green	Blue-yellow	Slate-orange	Black-white
Red-green	—	·09	·51	–·07
Blue-yellow	·09	—	·41	·14
Slate-orange	·51	·41	—	–·07
Black-white	–·07	·14	–·07	—

(iii) Beginning with highest speed, and correlating only the ten colour-weak subjects:

	Red-green	Blue-yellow	Slate-orange	Black-white
Red-green	—	·49	·05	0
Blue-yellow	·49	—	·53	·81
Slate-orange	·05	·53	—	·68
Black-white	0	·81	·68	—

(iv) Beginning with lowest rotation-speed, and including all subjects:

	Red-green	Blue-yellow	Slate-orange	Black-white
Red-green	—	·12	·51	·29
Blue-yellow	·12	—	·30	·51
Slate-orange	·51	·30	—	·37
Black-white	·29	·51	·37	—

(v) Beginning with lowest speed, excluding colour-weak subjects:

	Red-green	Blue-yellow	Slate-orange	Black-white
Red-green	—	·12	·57	·35
Blue-yellow	·12	—	·35	·47
Slate-orange	·57	·35	—	·27
Black-white	·35	·47	·27	—

(vi) Beginning with lowest speed, correlating only the ten colour-weak subjects:

	Red-green	Blue-yellow	Slate-orange	Black-white
Red-green	—	·11	·26	·44
Blue-yellow	·11	—	·44	·71
Slate-orange	·26	·44	—	·51
Black-white	·44	·71	·51	—

(vii) Average of the four trials (ascending *and* descending method) taken together, and including all subjects:

	Red-green	Blue-yellow	Slate-orange	Black-white
Red-green	—	–·09	·41	·16
Blue-yellow	–·09	—	·38	·26
Slate-orange	·41	·38	—	·27
Black-white	·16	·26	·27	—

It may be added, that among these ten 'colour-weak' subjects there were five, whom, according to the diagnostic suggestions in Prof. Stilling's work, I would class as red-green blind with normal sensibility for the brightness-value of coloured lights, and five whom I would call deficient in the blue-yellow, with lowered sensibility for (the brightness-value of) coloured lights. Whilst the former were unable to read quite a considerable number of the tables, the latter only missed a few.

V. DRAWINGS (SINGLE AND DOUBLE DESIGN).

This experiment was designed for the purpose of testing the inhibitory effect of Perseveration on purely visual form-memory. In order to exclude ideation as far as possible, drawings had to be chosen which were altogether unknown to the subjects and afforded as few helps as might be toward holding them fast in memory by any other means than the after-effect of the visual impression. After trying some other more regular figures recourse was had to shorthand signs (Gabelsberger system) variously modified to eliminate too great similarity with familiar letters or figures. It must, however, be stated that this was not successfully accomplished everywhere: there seems to be no conceivable drawing or design into which, or parts of which, no subject can read a letter, word, or to which one cannot give some sort of name by which to remember it. But many observers told me that though they did think of such helps during the exposure of the drawings, they had afterwards, when trying to reproduce them, completely forgotten both the design and the thought-of means of recall, or that the latter proved much too hazy and vague to be of use. Besides, special care was taken in arranging the system of marking to eliminate the influence of such ideational helps. The original drawing was covered with a sheet of tracing paper and on this little lines were drawn so as to divide up the entire design into ten parts of equal difficulty, making any portion the bigger, relatively, the more it tended to resemble some familiar figure or form. With this tracing placed upon the original, every copy made by the subjects was compared very carefully and on several occasions. If the respective portion was correctly reproduced, it counted + 1; if the reproduction was not fully satisfactory, though showing a considerable degree of similarity, it was marked + ·5; if it fell short of that, or was missing altogether, it counted 0.

There were on the whole 24 drawings, six for each sitting. In order to obviate any interference of preceding with succeeding exposures, the six designs of each day were given in two groups of three, with various other experiments between, so that the visual impression of the first group must have completely disappeared before the second group was brought in. The three drawings thus belonging together were pretty much of the same degree of difficulty and more or less alike, without, however, containing identical features (except in one or two pairs). In arranging the entire series an attempt was made to counteract the influence of practice and training by making every following group of three more involved and complicated than the preceding one. Of each set of three two drawings were designed to go together, being exposed alternately, first the left one for two seconds, then the right one for the same time, then again the left, etc., the whole exposure of the two lasting 12 seconds. As the alternate exposure of two drawings was meant precisely to cause interference, they had as much as possible to fall on identical portions of the retina, and therefore symmetry of one with the other was to be avoided. The third drawing of each group was exposed singly, to afford a measure of the subject's ability in drawing generally and in retaining and reproducing forms of that kind. This single design was exposed during six seconds; the first time (at each of the four sittings) after, the second time before, the two others corresponding with it.

The drawings, as a rule, consisted of one continuous line variously broken, curved, and involved, of equal thickness (about 2/3 cm.) throughout, drawn with ink on white cardboard. They were arranged in proper order behind pasteboard screens which were only removed for the actual exposure. As all 47 subjects underwent the experiment at the same time, some had to be at a greater distance from the original than others, and the illumination must have been much more favourable for some than for others, though all could see the drawings pretty well, as the lines were rather thick. The subjects were supplied with pencil and six slips of white paper, which they marked 1*l*, 2*r*, 3, 4, 5*l*, 6*r*, etc., according to the number of the original, and its position to the left, to the right, or its being exposed singly. The following was the instruction: "I am going to expose here some meaningless designs—first, two of them alternately, then one alone. Look at them very carefully during the few seconds of exposure. Then, when I say 'now,' and remove the original, you must draw each one from memory, as best you can, on the papers supplied to you. Do not copy from your neighbours, but work quite for yourselves and independently. And try to be quick, as you will have at most about a minute for each drawing."

As measure of interference, and thus of Perseveration, was taken the quotient Ms/Md, where Ms stands for the average marks obtained in the reproduction of the eight drawings exposed singly, and Md for the average marks obtained in reproducing the 16 drawings exposed two and two together. To be able to estimate the reliability of the experiment, the same quotient was worked out for the drawings used on the first and fourth, and on the second and third days respectively. The correlation is only $r = \cdot 23$ (36 of the 47 subjects being present in all trials).

The following table will illustrate the values actually obtained.

Rank of subj.	1	6	12	18	24	30	36	42	47
Ms	4·93	4·19	3·92	3·67	3·44	3·25	3·07	2·81	2·31
Md	3·21	2·68	2·50	2·25	2·09	1·96	1·75	1·62	1·09
Ms/Md	3·42	2·25	1·93	1·84	1·62	1·54	1·44	1·36	·98

The correlation of *Ms/Md* with the colour discs is: $r = \cdot 38$ with red-green, ·07 with blue-yellow, ·50 with slate-orange, ·24 with black-white, and ·50 with the average of the three coloured discs taken together, all according to the 'descending' method (highest speed at first). The corresponding figures for the 'ascending' method (slowest speed at first) are: ·09, ·09, ·35, ·35, ·12. The correlation of this experiment with the others is exhibited in the general correlation table.

VI. NARRATIVES.

To investigate the influence of Perseveration on the higher forms of memory, three pairs of narratives were selected for three trials, all a week apart. With each pair the first narrative was read to the subjects (all working together) twice in succession. They then wrote answers to about a dozen questions on it, each subject being supplied with his own copy of the set of questions; then, after removing their papers, the same story was read a third time to fix it more firmly in the memory. Immediately after that the second narrative was read once only, and the set of questions on it answered as before. In all cases the questions comprised 22 points, so that a subject answering all of them correctly would have gained 22 marks in each set. The quotient $M1/M2$, where $M1$ represents the marks obtained in the answers corresponding to the first narrative, and $M2$ in those of the second, was taken as measure of the interference-effect of Perseveration.

But on correlating the values of this quotient in the three weekly trials, their reliability proved hopelessly low: $r = \cdot 11$ for the first and second, ·04 for the second and third, and ·02 for the first and third. So they represent practically three entirely different experiments, the factor or factors common to all of them being overruled by some specific, peculiar factor or set of factors in each of them. On studying and comparing the various narratives and the results obtained more in detail, it became evident, that the two narratives of the first day had been much too easy, and here, as also in other experiments, Perseveration seems not at all to be involved in and brought to light by an easy task. Besides, on this first day the practice effect of the first on the second narrative and set of questions must have been considerable. The stories of the second day were much longer, and much more difficult; but they were both on the same hero and rather alike as to content and setting, so that the first largely served as preparation and help for the second: 26 out of 44 subjects actually scored higher in the second set of questions than they did in the first, though, as stated above, the second narrative was read only once, the first twice, and the minds were fresher on doing the first than on doing the second. None of these, or similar, disturbances could be detected with regard to the narratives of the third day. They were somewhat alike in general subject matter, but persons and scenery differed entirely, and the second was a little longer and rather more difficult than the first. Yet even here 11 out of 44 subjects obtained more marks in the second than in the first set of questions. The results of the first and second days were therefore discarded altogether, and only those of the third day were correlated with the other experiments.

Of course, special care had to be taken to find narratives entirely new to all subjects. These two were used on the third day:

(i) AUGUSTUS AND CINNA.

One evening in the year 4 A.D. the Roman emperor Augustus received the intelligence that a new conspiracy had been formed to take his life and to restore the republic, and that Cornelius Cinna, one of the noblest Romans, was the head of the conspirators. Augustus was grieved at the news and spent the whole night in thought and deliberation. Ever throughout these 30 years since he had succeeded in making himself sole ruler of the empire he had had to shed the blood of the most accomplished and influential men because of these endless attempts at his life. But what else could he do than condemn to death, in self-defence, those that wanted to bring about his own destruction? Finally, on the advice of Livia, his wife, he sent for Cinna and addressed him thus: "Remember that I spared your life, years ago, when I found you in the camp of my enemies. I restored to you your estate and granted you honours in preference to men whose parents had been my companions in arms. And you, in return, have conspired to murder me. Yet I again spare your life, and I ask you to be my friend henceforward, and to vie with me, which of us can show the nobler spirit, I in sparing your life, or you in acknowledging the favour." After this there was no further conspiracy for Augustus to suppress, and for the rest of his life he had in all his dominions no truer friend than Cornelius Cinna.

(ii) LOUIS THE BAVARIAN AND FREDERIC THE FAIR OF AUSTRIA.

The two young dukes Louis of Bavaria and Frederic, surnamed the Fair, of Austria had grown up together in closest companionship at the court of Vienna and had ever been the most intimate friends, when, in 1314, an event occurred which set them against each other as the leaders of two hostile armies on many a battlefield. Each of the two princes was elected German Emperor, Louis by the majority, Frederic by a powerful minority of electors. After eight years of war and bloodshed Frederic was defeated by Louis, taken prisoner, and confined in a lonely castle on the outskirts of the Bohemian Forest. Several years passed, during which Frederic's brother continued the war, without any real success, but to the great misfortune of the empire and the people. Louis all the while had not forgotten the friend of his youth. One day he appeared unexpectedly before the solitary castle where Frederic lay a prisoner. In the long interview that followed he reminded his defeated rival of the happy time, now so long past, when they would ride and hunt and fight side by side and could scarcely enjoy a pleasure, unless they shared it with each other. "Will then these happy days never return?" asked Louis, at last. "Be free, return home to Vienna, and persuade your brother to desist from this endless, useless war; and then come to me to Munich, be again my friend and share with me the honour and the burden of the imperial throne." Frederic gratefully accepted, and returned home. But he could not prevail on his brother and family to desist from the war. Unable to fulfil this condition, as he had understood it, of his liberation, he presented himself to Louis to be his prisoner once more. But Louis embraced him and asked him to remain at his side, his co-regent and his friend.

The corresponding sets of questions were:

(i) 1. When did Augustus receive the intelligence of a new conspiracy?
2. What was the aim of the conspiracy?
3. Who was Cornelius Cinna?
4. How did Augustus feel at the news?
5. How did he spend the night?
6. What had he had to do throughout 30 years?
7. Why?
8. Since which event?
9. Of what did he remind his enemy?
10. What did he propose to Cinna now?
11. What was the result of this act of Augustus?

(ii) 1. When did Louis and Frederic become enemies?
2. Why?
3. How long did the war last?
4. What happened then to Frederic?
5. Where was the castle?

6. Of what did Louis remind his enemy?
7. What did Louis propose to Frederic now?
8. What did Frederic do?
9. What could he not do?
10. How did he act then?
11. What was the result of his action?

It will readily be seen that several questions of the second set are partly identical with, and partly very much like, some of the first set, in order to give the after-effect of the first narrative a fair chance of interfering with the memory of the second. The following table illustrates the values (marks) obtained. (For subjects absent on the third day the average of the first and second was used.)

Rank of subject	1	6	12	18	24	30	36	42	47
$M1/M2$	2·0	1·77	1·41	1·31	1·11	1·02	·94	·85	·67

The correlation of this with the other experiments is given in the table on p. 416.

VII. ASSOCIATIVE REACTION.

Müller und Pilzecker (*Die Perseverationstendenzen der Vorstellungen*, p. 62) speak of "habituelle Aushülfesilben," reaction syllables recurring again and again as substitutes for the correct, but at the time forgotten, syllables in the process of testing meaningless syllables read or learned previously. This suggested a further method of studying the effect of Perseveration. For a considerable time the ordinary method of associative reaction was tried. Long lists of stimulus words were prepared and given to a fair number of subjects, the reaction words recorded and the reaction times measured with the Hipp. The subjects had no other task than to react with the very first word that came to their mind on seeing the stimulus word, and to be as fast as they could. The lists of stimulus words were so arranged that several words recurred repeatedly at different intervals, whilst most of them came up but once. It was hoped that thus various indices of Perseveration could be obtained: the number of identical reactions, the absolute length of reaction time, and the difference in length of reaction time for words recurring oftener and at short intervals, and for such as occurred once only. But no definite, reliable result was arrived at. The reason of the failure seemed to lie in the various pre-existing associations connected with the stimulus words, associations which must have been rather different with different subjects, and of very different degree of firmness and readiness.

Words with meaning therefore were discarded, and meaningless syllables used instead. The instruction for the subjects remained the same as before: they had to react with the very first word (not meaningless syllable) that came to their mind on seeing the stimulus syllable, and to try to be as fast as they possibly could. For what was aimed at was some insight into the spontaneous working of the mind, not into the result of deliberation or choice. Yet again the results turned out too vague and indefinite to repay the time and labour involved in carrying out and evaluating the full set of experiments, with a great number of subjects.

Hipp and lip-key were accordingly put aside; and a simple apparatus was constructed, by means of which the reaction words of many subjects could be obtained in a shorter space of time, together with a rough measure of the reaction time for the whole series of words or syllables.

This 'Reaction-Box' consisted of a sort of tin box, about 20 cm. long and broad, and 3 cm. high, with two wooden rollers inside, over which ran a piece of canvas. The top roller was provided with a handle for turning it and moving the canvas, and the strips of paper were stuck on to the latter, upwards or downwards. A pair of springs kept the cloth fully stretched, so that it readily followed the slightest turn of the handle. From the upper cover of the box was cut out a slit nearly across its full breadth. It could be entirely or partially closed by six little shutters turning on hinges. As a rule only two of these shutters were opened at a time, the one in order to expose the stimulus syllable, the other, to write in the reaction word. The subject himself easily worked the whole apparatus,—he had but to slightly turn the handle to make the stimulus syllable come up, write his reaction word in the blank space of the second opening, and then turn on again to make both disappear together. Thus only one syllable was exposed at a time and the subject at once lost sight of his own reaction word. The instruction was to write down the very first word that came to his mind on seeing the stimulus syllable, and to be as fast as he possibly could. As it was noticed, in the course of a first set of experiments, that one or the other subject tended to pay too little attention to the stimulus syllable, and rather to simply write down a list of words as they mutually suggested one another, than to react to the successive stimuli (in such cases the reaction time became much shorter, 35–40 seconds for 22 words), in the final set of experiments the instruction was to read the syllables aloud. A better procedure still would seem to be to require the subjects to first copy the stimulus syllable, and then write their reaction word beside it; also to give the stimulus syllables not printed or typewritten, but in a somewhat difficult and not easily readable handwriting: the more the experiment draws off the subject's attention and deliberate working of his mind to something else, the harder the task is made; and the more completely all his mental energy is engaged, the more readily Perseveration would seem to become manifest.

In the final set of experiments 44 stimulus syllables were given in two sets each of 22. The time required for writing the corresponding reaction words (*i.e.*, for the 22 words of each set) was taken with the stop-watch. The experiment was done three times (each time with a fresh list of stimulus syllables) by every one of the 47 subjects; they all came in singly, nobody being in the room at the time but the experimenter and one of them. They were all told that it mattered nothing what word or kind of word, noun, verb, adjective, etc. they might give, provided only they reacted as fast as possible and gave a word which had meaning in any language they happened to be familiar with.

Yet even so the experiment proved too difficult without preceding practice. With many a subject consciousness seemed completely blocked by the one syllable exposed and read; they would repeat it again and again, look about, make gestures, yet be unable to find any word, though for the onlooker many a word seemed to lie at hand. For instance, one stimulus syllable was 'keb.' One subject (a strong Perseverator) repeated the syllable aloud several times, gesticulated vehemently, but for the space of some 20 seconds could find no reaction word whatever, though "cab" lay near enough. Such delays recurred frequently, not only in the first sitting. Many subjects were inclined to give up altogether and turn to the next stimulus, whenever the reaction word did not come readily. To prevent this, and also to save time, they were allowed from the second sitting on, to react with meaningless syllables, in case they could not find a meaning word. But this seems to have spoiled the result; the time indeed became shorter and the delays fewer, but just the 'recurrent identical reactions' largely fell away, the whole procedure became too easy.

In the general correlation table only the time factor is utilised, *i.e.*, the number of seconds required by the subject for one reaction, on the average; and only the results of the second and third day are taken, the first experiment being considered as mere practice. Thus the values actually correlated with the results of the other experiments, and exemplified in the following table, are obtained by first dividing the time taken by the subject for writing down his reaction words (44, if he did not skip any) by the number of reaction words actually given by him, and then averaging the figures arrived at thereby for each one of the two trials. The reliability of this factor is $r = \cdot 90$. The following may illustrate the values obtained. The slowest subject ranks first.

Rank of subject	1	6	12	18	24	30	36	42	47
Seconds pro react.	8·8	4·7	4·1	3·7	3·3	2·8	2·4	2·0	1·6

VIII. Essays (Comparisons).

It seemed *a priori* probable that Perseverators would do better than non-perseverators in any long task, when they had time enough to work themselves into a kind of set for it, and worse, in short performances involving rapid change from one subject to another. To investigate this, and if possible, to find an index of Perseveration in the higher and highest, least sensory, activities of the mind, 25 very difficult 'problems' or themes were selected, to be treated one after another, five at each sitting, allowing for some four minutes, for others only 40 seconds time. But it proved impossible to find themes which were of equal difficulty for all subjects (15) that took part in the experiment, and so either general intelligence, or specific knowledge of the precise subject-matter of the problem completely swamped and obliterated the effect of Perseveration.

Accordingly, in place of the hard and highly abstract themes, I substituted a set of 12 comparisons of familiar and very different subjects. Three were used for each sitting. Everyone that took part in the experiment knew enough of the topics to write an essay on it, if he only could succeed in recalling to mind what he knew, during the short time at his disposal. Four minutes were allowed for the long, forty seconds for the short tasks. Some of the themes were: England and Ireland, Wellington and Napoleon, Railway and Telegraph, the Thames and the Rhine, etc. The subjects were instructed to be as fast as they could, to get on the paper as good an essay as possible: therefore not merely to put down anything that came to their mind; for they were told that statements not to the point would be marked 0, whilst such as were just barely satisfactory would receive + 1, such as were 'good' + 2, and every statement particularly appropriate or very good would obtain + 3. Besides, all the essays had to be real comparisons of the two subjects mentioned in the title, not mere descriptions of either of them. The long and the short tasks alternated with one another. On the first and third days there were two tasks of 40 seconds, and one of 4 minutes, and on the second and fourth days one of 40 seconds, and two of 4 minutes.

As a measure of Perseveration I at first tried the quotient $Ml/6Ms$, where $Ml/6$ stands for the average marks or efficiency pro 40 seconds in the long or 4 minutes tasks, and Ms for the average efficiency in the short or 40 seconds tasks; the greater the value of this quotient, the greater is Perseveration. This factor gave the correlations $r = \cdot 09, \cdot 12, \cdot 02, \cdot 07, 0, \cdot 19, \cdot 26$ with Rate of Tapping, Letterwriting, Cancellation, Colour discs descending method, Colour discs ascending method (average of the three coloured discs in both cases), Drawings, and Narratives respectively. Somewhat higher, on the whole, are the corresponding correlations for the difference, $Ml/6 - Ms$. The reason why here, and here alone, the difference gives a better measure than the quotient is perhaps the fact, that here the range between the highest and lowest value is not great, all the subjects having pretty much the same chances, just the opposite of what had been the case with the problems. The following is an illustration of the figures obtained.

Rank of subject	1	6	12	18	24	30	36	42	47
Value of $Ml/6$	3·28	2·72	2·49	2·29	2·10	1·99	1·87	1·68	1·35
„ „ Ms	4·00	3·40	3·00	2·75	2·50	2·17	1·92	1·67	1·00

IX. INTERROGATORY ON PERSEVERATION-TENDENCY.

Read the following questions several times at your leisure, and write a short, but clear answer (simply a 'yes' or a 'no,' 'very much,' 'never') on the respective line.

1. Do you often notice a tune, line of poetry, phrase, problem, etc., coming back to your mind again and again without your intending it?

How often (about) a week? At what time of day more frequently?

2. Are your dreams more commonly about some past experiences or events? Or rather about things scarcely ever thought of before?

3. When something is to be done or imminent, *e.g.*, a task, an examination, etc., does it often come to your mind during the days preceding it?

(4. Do you easily forget little engagements?)

(5. Do you find it easy to change quickly from one task or kind of work to another?)

6. When writing an essay, or working out a problem, do you find it easy to interrupt it? Or do you feel a strong tendency first to finish it in spite of fatigue?

7. When you have to interrupt it, does your attention easily pass on to other things? Or do the thoughts of the essay or problem keep coming back to your mind?

8. On taking it up again after the interruption, do the former thoughts readily come again? Or have you almost to begin anew?

(9. When studying, or reading, are you easily disturbed by what goes on around you?)

10. When unexpectedly addressed or asked a question which you know well enough, but have not been thinking of at the time, can you answer readily and quickly at once?

11. Which would you like better, to pursue one subject of study, one kind of work, thoroughly and fully, or to have several things on hand?

12. Which would you like better, to go on in the same familiar occupation, place, companionship, etc., or to have frequent change?

13. Do you, after leaving (for a longer time or for good) a place, room, occupation, etc., feel as it were homesick after it?

14. (*a*) When you have once begun something, or done it a few times (gone away, played a game), do you feel a tendency to stick to it, to do it again and again, though you have no longer any reason for it? (*b*) Or even against reason?

(15. (*a*) Does it come easy to you to have fixed hours for work, play, etc.? (*b*) Fixed places for books, instruments, etc.?)

16. When stepping off a train before it has completely stopped, or sitting in a train when it starts or stops, do you feel a considerable shock? Or do you scarcely notice any shock at all?

17. (*a*) Do you, after a long railway journey or sea voyage, seem to hear the noise and feel the motion of the train or ship for some time? (*b*) Have you ever noticed it recurring in your dreams?

A copy of this interrogatory was given to all the subjects; they took it home and had practically as much time as they liked for filling in their answers. None of the questions were placed within brackets; these are only added here to mark off the questions finally disregarded in marking and correlating.

The replies given by the 47 subjects were at first marked in the simplest possible way, by assigning the mark + 1 to every answer affirmative of what seemed to be perseveration-quality. Thus a 'yes' to question 1, or 3, or the first part of 2 (the alternatives were only put in to prevent suggestion), or a 'no' to No. 4, or 5, etc., counted + 1, every 'very much' added to it + ·5 (in the second method of marking described below, + 1), every 'sometimes' and the like limitations subtracted ·5 from the 'yes.' Besides, in question No. 1 the frequency-statement 'more than 4 times a week' raised the + 1 to + 2, and 'more than once a day' to + 3. The subjects were then ranked according to the sum of marks thus obtained, and this gave a correlation of $r = \cdot 19$ with the pool of tests 1, 4, 6 (as given in the general correlation table, p. 416), and $r = \cdot 35$ with the pool of the other three tests 2, 3, 5.

But it remained after all a mere assumption that the questions asked in the Interrogatory should really all bear upon Perseveration, and all in the same degree, justifying the method of marking them all equally, not giving more weight to some than to others. To gain a little insight into this point, I drew up a table comparing the answers returned to each question by the first ten Perseverators (according to their rank in the eight experimental tests taken together, *i.e.*, by averaging the ranks of each subject in the eight tests, and ranking them again on the basis of the value thus obtained) with the corresponding answers of the ten most pronounced Non-Perseverators. Adding up the number of marks obtained by each group for each question, it became evident that the questions did not all test perseveration equally, some perhaps not at all. In questions No. 4, 5, 9, 15*a* and *b* the Non-Perseverators had actually got more marks than the ten 'Perseverators' (6·5, 2, 3, 6, 9, against 5, 1·5, 2, 5, 7, respectively). These four, or five, questions were omitted altogether in the second method of marking. In another group of questions, No. 10, 11, 13, 14*a* and *b*, 16, 17*a* and *b*, the Perseverators scored decidedly higher than the Non-Perseverators (7, 6, 6, 8, 6, 6, 10·5, 5, against 4·5, 3, 2, 6·5, 4·5, 0, 7, 3). Consequently the answer affirmative of Perseveration to these questions was then marked + 2, as more surely indicative of true Perseveration-tendency. The rest of the questions showed only a very slight superiority in the number of marks of the Perseverators, or equality; and here the marking was left as before, + 1 for every assertion of the Perseveration-quality.

The ranks of the 47 subjects on the adoption of this second method of marking correlate $r = \cdot 86$ with those obtained by the first method, a sign, as it would seem, that thereby not too much of arbitrariness, not too much of artifice had been introduced. The correlation of the Interrogatory, evaluated according to the second method, with the individual tests and with the 'pools' is shown in the general table (p. 416).

X. ESTIMATES (OF 'PERSISTENCE' QUALITIES OF CHARACTER).

Of the same 47 subjects who underwent the experimental tests of Perseveration described in the foregoing pages, Mr E. Webb collected careful estimates as to their Persistence-qualities of character (cf. his work).

The subjects had been divided into seven groups, and two judges were appointed for each group to study independently the behaviour of the individuals entrusted to them and to assign them their marks for eight Persistence-qualities. The highest mark a subject could get for any quality was + 3, the lowest − 3. The marks thus allotted to each individual were added up, and then the 47 subjects ranked according to the total. Needless to say, the judges making the estimates knew absolutely nothing of the results of the experimental tests, which at the time were not yet even worked out; and the experimenter did not receive the result of the estimates until he had completely finished his own task of marking and correlating his tests. It must be confessed, though, that a strong positive correlation between tests and estimates was expected.

It turned out otherwise. The correlations really obtained are given in the following table.

General Correlation Table.

	1 Tapping	2 Letter writing	3 Cancellation	4 Colours, descending	5 Drawings	6 Narratives	7 Assoc. React.	8 Essays	9 Interrogatory	Estimates
1 Tapping	—	·26	·07	·39	·23	·21	·24	·14	·09	−·35
2 Letter writing	·26	—	·31	·27	·26	·40	·39	·16	·29	−·05
3 Cancellation	·07	·31	—	·40	·16	·16	−·05	·02	·51	−·27
4 Colours, descending	·39	·27	·40	—	·50	·11	−·05	·12	·29	0
5 Drawings	·23	·26	·16	·50	—	·32	·26	·24	·12	0
6 Narrative	·21	·40	·16	·11	·32	—	·18	·21	·26	−·12
7 Assoc. React.	·24	·39	−·05	−·05	·26	·18	—	·02	·11	−·37
8 Essays	·14	·16	·02	·12	·24	·21	·02	—	·18	0
9 Interrogatory	·09	·29	·51	·29	·12	·26	·11	·18	—	−·07
Estimates	−·35	−·05	−·27	0	0	−·12	−·37	0	−·07	—
Colours, ascending ...	·31	−·02	−·02	·34	·12	·27	·14	·02	·02	−·04

The general tendency of these correlations comes out most clearly by pooling all the tests together. This pool shows a correlation with the interrogatory of + ·41, but with the estimates one of − ·26.

SUMMARY OF RESULTS.

It will readily be allowed, I think, that the experiments described differ widely from one another and cover, together, a pretty large range of mental functions; also, that the Interrogatory filled in by the 47 subjects contained all the features of Perseveration pointed out, thus far, in the literature on that trait. It correlates positively with every one of the tests. So do all the tests among themselves, in spite of the poor reliability of the *P*-factor which had to be taken as measure, in five cases a quotient or difference, dependent on the mutual relation of two variables at a time. Professor Spearman has had the kindness to re-test all correlations by having them worked out according to the Product-Moments method, and has arrived at substantially the same results. To obtain some insight into the mutual relation of the various parts of the Interrogatory and its reliability, the 14 questions allowed to stand in the final marking (cf. p. 415) were divided into four groups, according to the nature of the mental process or function they seemed to represent:

(1) Questions 1–3: spontaneous reproduction of more remote experience, or ideational level.

(2) Questions 6, 7, 8, 10: immediate after-effect, and inhibitory aspect of Perseveration in intellectual activity.

(3) Questions 11–14: conational and habit-forming aspect.

(4) Questions 16 and 17: purely sensory and motor aspect.

The correlation of the four groups among themselves turned out very low, so that the positive correlation of the whole Interrogatory with all the tests becomes the more significant. This is shown by calculating what may be called the 'true' general correlation between the tests and the Interrogatory. We may conceive the research to be greatly extended, so as to include extremely numerous tests similar to those used here and also extremely numerous interrogations. It is possible to evaluate the correlation that would occur between the pool of all the tests and the pool of all the questions. (See this *Journal*, 1913, v. p. 421.) It proves to reach just about plus unity, that is, perfection. This means that the factor producing the correlation between the tests must be taken as being identical with that producing the correlation between the questions.

It is similarly possible to evaluate the correlation that would occur between the pool of extremely numerous tests, or questions, and the estimates. In either case, it comes to about $-{\cdot}40$.

Therefore, in spite of the lowness of nearly all single correlations, they must be the result of a general factor influencing all the various performances tested by the experiments and by the Interrogatory. And this general, common factor cannot be general ability, which will scarcely be supposed to influence, *e.g.*, Tapping, or Colour Discs, or the various functions tested by the Interrogatory, and which in all other experiments (except only Associative Reaction) would needs be eliminated in the result, because equally entering into numerator and denominator. In the Essays the factor Ml (*i.e.*, the average marks obtained in the long, four minutes, tasks, where general ability will be of greater influence than in any other experiment) correlates slightly negatively ($R = \cdot 16$, without inverting ranks) with the P-factor $Ml/6 - Ms$. Therefore Perseveration may justly be claimed as this one common factor. The lowness of the single correlations tends to show that normally, with average, unselected subjects, this general factor, though present and operative, is not very strong[1].

But this Perseveration, as tested by both experiments and Interrogatory, does not correlate, or correlates slightly negatively, with the Perseveration-, or Persistence-, or Perseverance-qualities of character and behaviour. Why? The experimenter must confess that he felt disappointed and somewhat puzzled on arriving at this result. But on studying his own, and Mr Webb's methods and figures more closely, he came to the following solution, which, if corroborated and confirmed by further experiments and observations, would seem to be of great interest for general Psychology.

The Perseveration tested by the experimental methods described above, and by the Interrogatory, is a native quality of the nervous system, innately different with different individuals. The Perseveration or Persistence tested by the estimates of qualities of character and behaviour is the result, not of nature and the native system alone, but of that and the individual's own effort and will. The absence of correlation between the two P's proves the independence of innate Perseveration and Will, and the slightly negative correlations tend to show that the self, with persons used to act morally, from higher motives of reason and principle, not according to merely natural bent and inclination—the 47 subjects are young men training for teachers—can modify, and directly counteract, its own nervous system and its innate tendency towards Perseveration or the opposite. The proposed solution could, I think, be tested by trying both the experiments and the estimates on two very different groups of persons, the one group of such as are used to act 'against the grain,' and of others who quite habitually 'let themselves go'; with the former group I would expect a negative, with the latter a positive correlation of the two *P*'s.

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[1] The following personal observation may be of interest in this connection: On returning from India to Europe in October, 1907, much weakened by the Bombay climate and ill-health during the preceding Monsoon, I was struck by the exceedingly strong Perseveration effect of the movement of the ship during my sleep. The sea had been very calm all through the 20 days of the voyage, and the rolling and rocking of the vessel had been very slight. Nor had I been sea-sick. Besides, I had noticed nothing during the nights on board and on the railway. But when sleeping again in an ordinary, steady bed, I nearly at once, and continually during about the first eight nights, dreamt of being rocked to and fro as if in a small boat tossed about by the waves. I often awoke in perspiration, but only to find myself once more in the same 'shaky' situation as soon as I fell asleep again. The feeling never came when I was awake. There had been nothing like it after my voyage from Europe to India 11 years ago, nor has the experience recurred now, after my return-voyage to India (5th to 20th July), though during the last five days the vessel rolled very much, owing to the rough Monsoon weather. But am now, and was 11 years ago, in a far better condition of health and strength than on the first-mentioned occasion.

Some slight test of this solution could already be gained with regard to some of the 47 subjects who served in both experiments and the estimates. Four subjects, out of the 47, can be said to rank high in both experiments (average of tests 1–6) and estimates: the 1st, 7th, 9th and 13·5th according to the estimates are 11th, 8th, 14th, and 7th in the experiments. Four ranking very low in the estimates, *i.e.*, being 38·5th, 47th, 29·5th, and 46th, are rather high according to the experimental tests: 6th, 11th, 2nd, and 1st respectively. And with five the opposite is the case: the subjects 16th, 5·5th, 2nd, 13·5th, and 4th in the estimates are 43·5th, 42nd, 47th, 46th, and 35th according to the experiments. These subjects were estimated once more by several independent judges, who knew nothing whatever about the theory in question, as to strength of will, natural endowment, and the use they could be said to have made of the capacities of their system. The first group, who ranked high in both *P*'s, were returned as foremost in the three points; the second group as deficient in strength of will and the use made of their opportunities; the third as practically the opposite.

Accordingly, the proposed results and interpretation would seem at least worth while re-testing by further experiments and observation, especially because of the hopeful outlook they open up for every one interested in education and formation of character, and the real value and possibilities of the human personality. Man can make something of himself, in spite of what nature and inheritance have endowed him with.

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THE FORMATION OF PROJECTED VISUAL IMAGES BY INTERMITTENT RETINAL STIMULATION.

I. GENERAL CHARACTERISTICS OF THE IMAGE.

By GEORGE H. MILES.

I. Introduction.
II. Method of producing the projected image: observations of its chief characteristics.
III. Part played by intermittent stimulation.
IV. Observations concerning the projection of the image.
V. Suggested explanations.

I. INTRODUCTION.

It is a well-known fact that when the eyes are closed after a bright light has been fixated, an after-image of the object can be seen. A similar image can also be seen if the eyes are directed to a uniformly illuminated background, immediately after the fixation. These images gradually fade; but if light is allowed to fall intermittently at a suitable rate on the closed eyelids, or if the intensity of the illumination of the screen on which the image is projected be varied at a regular rate, the following changes are observed. In each case, the fading and indistinct image is rapidly revived; it becomes clearer in outline; its intensity and objective character are strikingly increased; its duration is prolonged; and in many ways the image thus aroused forms such a marked contrast to the ordinary image that further investigation of its peculiarities is demanded. An attempt has been made to discover the main factors involved in the production of the projected image. In this paper a brief account is given of a few of the experiments that have been carried out with this aim in view.

Even in the simplest experiments it was evident that, at a certain rate of stimulation, the image became much clearer than at any other rate. In a subsequent paper, details will be given of apparatus which was used to produce the image under carefully controlled conditions, and of methods which were adopted in a preliminary survey to determine the best rate of intermittent stimulation and the ratio of stimulus to rest most effective for the production of clear images.

Throughout the experiments colour changes were observed in the image after all but the shortest preliminary exposures. These changes were so clearly marked, and the image presented such an appearance of objective reality to some of the subjects who took part in the preliminary tests, that a systematic testing of a greater number of subjects was undertaken. In these experiments great individual differences were noted in the sequence of changes and also in the time of retention of the image. Over forty of these subjects had been previously tested for immediate visual memory, and on comparing their records for these memory tests with those obtained in connection with experiments to determine the time of retention of the image, it was noted that a marked relation existed between the two abilities under certain conditions.

It is hoped that the accounts of the work done will challenge a fuller investigation of the factors underlying the production of the projected image; and will suggest the utility of the method of intermittent stimulation in investigating certain visual phenomena.

II. METHOD OF PRODUCING THE PROJECTED IMAGE: OBSERVATIONS OF ITS CHIEF CHARACTERISTICS.

The fact that a faded after-image can be revived upon renewed stimulation by weak, diffused light, has been frequently noted.

The following cases are typical instances:

McDougall[1] states: "If after fixating any patch of bright light on a dark background for thirty or sixty seconds, I completely exclude all light, and watch the after-image, it will disappear completely if all light be kept from the eyes, perhaps after two or three short periods of invisibility. If then a very little light be admitted to the retinæ for a few seconds (to expose the closed eyes to diffused daylight is often sufficient), the after-image will invariably recur for a short time and fade again after a few seconds when all light is excluded.

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[1] *Mind*, N.S. x. 55.

If the original after-image be a bright one, it may be revived many times in this way and at each reappearance it is rather fainter than before, until it can be no longer revived by any means." Again, Titchener[1] gives as an experiment: "Seat yourself before a brightly illuminated window which is partly covered by a shade and through which a gate, trellis-work, shrubs, etc., can be seen. Secure the head in a head-rest and fixate the window. Cover the eyes with the hands and keep them covered till there is no trace of the after-image in the dark field. Maintain the original fixation as nearly as you can. When the field is clear draw the hands rapidly aside without any jar to the head or eyes and bring them as rapidly together again....The details of the scene, leaves of the shrub, string tassel of the window shade, etc., come out with surprising clearness."

In these experiments, the image produced by the admission of light gradually fades away. If, however, light be suddenly admitted a number of times in succession, by moving the hand or a screen between the source of light and the closed eyes, the image can be maintained for a considerable time, and each time the light is admitted there is a considerable increase in intensity of the image. It will also be noticed that a certain favourable rate of stimulation exists at which the successive images appear to fuse, and at times when the rate is successfully maintained the resulting image is remarkably clear.

A more striking example of the revival and maintenance of the image by intermittent stimulation, is seen in the following case. The eyes are directed and fixated for an instant on the disc of the sun when not too bright. On turning the eyes to another part of the sky, no image whatever may be seen if the exposure was short, but on blinking the eyes rapidly, a most distinct image, which is sometimes coloured, appears. This image can be projected in any direction by a suitable movement of the eyes, and on converging them the image can be brought from a distance to within a few feet of the eyes, where it remains so long as convergence is maintained, apparently in the air, with every appearance of objective reality. These marked changes which follow on the intermittent stimulation seem to indicate the need for a further investigation of the peculiarities of the image produced in this way.

The following experiments performed under bright adaptation and with foveal vision serve to bring out other peculiarities of the image and can be carried out under more convenient conditions. Though they are extremely simple, I believe that they point the way to more accurate work which should give a rich yield of facts in connection with the action of the retino-cerebral tract.

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[1] *Experimental Psychology*, Students' Manual, I. 28, Exp. 10; Instructors' Manual, I. 46, Exp. 10.

The simplest way of making these experiments is to sit with the back to a source of light and then fix the attention on a well illuminated sheet of paper placed at a distance of about two feet from the eyes. If now a filament of an electric bulb be fixated and a momentary flash be given, a distinct positive image will be seen on looking at the sheet of paper. This image dies away rapidly, but if the hand or a screen be moved rapidly between the eyes and the illuminated paper, a further series of bright after-images appears each time the paper is exposed. The images appear to equal the original in intensity, are much brighter than the ordinary after-image seen with closed eyes, and can be repeated for five or ten minutes according to the intensity and duration of the original exposure.

After prolonged exposure to light the resulting projected image is invariably coloured, and though the colour changes are best studied by using a bright uniformly illuminated disc as a stimulus, earlier experiments carried out with a bright filament present several points of interest. Though the sequence of colours has been found to vary in each individual, the following outline of the experiments dealing with short exposure first will give an idea of the character of the changes that take place.

Short exposure (about 0·5 sec.): In this the filament image appears immediately and is apparently of equal intensity to the original. It gradually diminishes in intensity, reversals take place at irregular intervals till the final disappearance as a black or grey image. Throughout, the image is exceedingly clear and well defined in great contrast to the after-image proper. The black negative image is often indistinguishable from a black printed hair line.

Medium exposure (about 15 sec.): The filament appears almost immediately, but is rapidly tinged with red and appears as a hot wire. Later it is outlined in black or dark purple till at last this is further outlined in green. Finally, a vivid green image appears and alternates with the red; green invariably appearing last and sometimes of a very vivid hue.

Long exposure (60 sec. or more): For so long an exposure as this the eye is with difficulty kept steadily directed towards the filament and in the projected image it is often possible to trace the eye wanderings by a number of images slightly outside the main image. It is partly on this account that the image appears much coarser than in the previous cases.

At first no image of the filament can be seen, but the bright surrounding metal of the bulb and the reflections in the glass, appear as a fairly well defined image and present a distinctly purple or lilac colouring. In the course of time the filament appears, first faintly and surrounded by a dark outline and then after a time varying according to the length of the original exposure, increases in brightness till it appears to exceed that of the paper. If the exposure has been sufficiently long, the image appears distinctly yellow. The edges of the filament now begin to show a reddish tinge on the inner part, changing through purple to a faint green at the edge of the image. The whole image changes periodically to a dark purple or grey, and after each reversal the green outer margin increases in intensity and extent. In time the light green colour extends over the whole image, increases in intensity, and sometimes with a favourable rate of intermittency, which is in general slower than for the positive image, appears as an exceedingly vivid streak of green, and fading, finally vanishes. If the exposure has been very prolonged this final image is sometimes of a definite blue colour.

III. THE PART PLAYED BY INTERMITTENT STIMULATION.

Throughout the literature dealing with after-images there is frequent reference to the projection of the negative after-images and the influence of the field upon which they are projected has been carefully noted. The case of the projected positive image has received less attention and the results obtained are somewhat conflicting, though it is generally accepted that the usual effect of a dark field for projection is to give a positive image, whereas, with a bright field, the image becomes negative. Whatever may be the final conclusion, the essential agreement that light reflected from the background upon which the image is projected, plays an important part in its formation, indicates the necessity for making a distinction at the outset between this image, which throughout this paper will be termed the 'projected image,' and the after-image proper, which is solely a result of continued activity in the visual tract.

The following experiments indicate the part played by the stimulation due to the illuminated screen and serve to bring out further differences between the projected image and the after-image proper.

A sheet of paper was prepared in the following manner. Strips of black paper were pasted in parallel rows at a distance apart of 8 cm.

and of a width of 4 cm., and at right angles across these another set was fixed making a grid of black lines 4 cm. wide with spaces of 8 cm. By the side of these was pasted a square of black paper of equal area.

This screen was brightly illuminated, and was viewed through the openings of a cardboard disc, revolving at such a rate that the paper was exposed for about one-twentieth of a second as each opening passed. The white-hot filament was fixated for about two seconds, and the eye was directed to the grid seen through the revolving disc. The projected image appeared on the sheet and the filament spread right across the black lines of the grid; and continued to do so for a considerable time till the image finally became negative. The image appeared quite as bright in those parts of the grid where there was but little reflected light as on the white parts of the screen. It was thought that there might perhaps be sufficient light from the black lines to give rise to the projected image over these. But in a subsequent test the eyes were turned to the black paper by the side of the grid. From this, however, no image could be obtained. Hence the formation of the complete image across the grid appeared to be due to other factors than direct stimulation of the previously excited area.

To test this question still more rigidly, a dark box 75 cm. × 80 cm. was prepared, with a circular aperture 8 cm. in diameter and across this a ground glass screen was placed, having in the centre a tinfoil disc, blackened on the exposed side which faced the inside of the box. Behind the box was placed an incandescent burner, in front of this a lens and further, a revolving disc so arranged that light from the burner illuminated the ground glass disc practically simultaneously, as an adjustable aperture in the disc passed the light. The disc was revolved at a constant rate so that the illumination of the screen took place regularly at about the rate of eight per second. Black velvet was arranged in the box so that no reflected light entered the eye which was situated at a distance of 55 cm. from the screen. The observer wore a black velvet mask and viewed the screen through two small blackened apertures so that as far as possible no light but what came directly from the screen entered the eyes. The observer now fixated a bright disc illuminated by light from an incandescent burner, transmitted through ground glass for five seconds. The disc was 7 cm. in diameter, and placed at a distance of 55 cm. Immediately afterwards the observer placed his head in the dark box and having fixated the black disc seen dimly on the screen, was covered with a cloth so as to completely exclude light from the box. An after-image appeared and faded. A shutter which had prevented light from falling on the screen of the dark box was now removed and immediately the first few flashes appeared, no black disc could be seen on the intermittently illuminated screen of the dark box. In its place was a disc, brighter than the surrounding screen and much brighter than the after-image previously seen. In a few seconds the image moved slightly and the black disc appeared as it were from behind the bright disc.

The image of the black disc on the screen would completely cover that part of the retina previously stimulated by exposure to the light of the bright disc. Hence this part was completely screened from the action of the intermittent light. The remainder of the retina was, however, fully exposed to the intermittent light from the screen and gave rise to the appearance of a bright disc in the middle of the uniformly illuminated screen and formed over the opaque black disc. This experiment in conjunction with a number of previous experiments made under less rigid conditions seems to show that the intermittent stimulation of the whole retina plays a part in the arousal of the projected image, and that the image can be formed in the absence of direct stimulation if the neighbouring parts of the retina be stimulated.

In order to test whether a reduction of the extent of the retinal stimulation would affect the character of the image, triangular strips of black paper, 1 cm. wide at their base, 15 cm. long and narrowing down to a point, were placed with their bases in a line so as to form a comb or series of gradually narrowing slits in which, when viewed by intermittent transmitted light, the bright area diminished towards the base. On looking at this after a flash from the filament, it was noticed that the image of the filament appeared as if behind the black lines when it was near the base, but appeared to cross them when the attention was directed to a point higher up. On an average it appeared to cross only where the illuminated surface was at least one-third of the total area, at a distance of about 5 cm. from the base of the comb.

This last experiment, in conjunction with others of a similar type, seems to point to the fact that the conditions for producing the brightest projected image require the stimulation of a considerable portion of the retina, and the fact that the image can be obtained when there is no appreciable direct stimulation of the retinal area originally excited, as in the experiments with the grid, comb, and disc, points to the fact that the whole retina is concerned in the formation of the projected image.

If the image obtained after a long exposure be projected on a screen which has black strips of paper arranged like the comb previously mentioned, the image appears to run down to the base of the comb where there is only slight illumination and is with difficulty brought back to the brightly illuminated part of the screen. It seems as if the nerve energy, discharging readily into motor paths, reflexly brought about the turning of the injured part away from the action of the intermittent light.

The black lines of the comb appear in this case to prevent the spread of the image and the image appears as if entirely behind the comb. If, however, the image be very carefully fixated, the light part can be seen to spread gradually across the black bar from the bright wings of the image. This part, generally of much less intensity, frequently appears as of a red colour.

With prolonged exposure to an intense light the fatigue of the whole system prevents the appearance of any image. A small black dot placed as a fixation point, in the centre of the screen is invisible when an attempt is made to fixate it, yet on turning the eyes to any other part of the screen no dark patch appears. It seems as if a foveally situated 'blind spot' had been produced which, though not showing as black in the absence of sensation, is yet incapable of assisting in sensing the black fixation point. When, however, at a later stage a dark image appears, the black spot can be distinctly seen through the dark image.

This in conjunction with previous experiments seems to point to fatigue at higher levels on exposure to intense light in addition to retinal fatigue. It has been shown previously that the projected image can be produced without direct stimulation of the originally excited retinal area. If in the last mentioned experiment mere fatigue of the retinal area were present, there would have been sufficient 'spread' of impulse from neighbouring areas to have given rise to the image. The gradual appearance of the image would therefore seem to be due to the gradual recovery of the fatigued tracts.

The foregoing experiments seem to bring out the following facts. The 'projected image' noticed on first directing the eyes to an illuminated surface, appears to owe its revival to the restimulation of previously excited tracts and is also influenced by the excitation of the remainder of the retina and more particularly by the parts bordering on the previously excited area. The influence of this excitation is most effective in the initial stages and its effect rapidly dies away. Intermittent excitation causes a constant succession of these changes and when given at a suitable rate, results in the fusion of the most effective phases with the result that the projected image produced by intermittent stimulation shows a marked gain in intensity. Although these additional factors may justify a differentiation between the projected image and the after-image, from a physiological point of view, a mere increase of intensity alone is insufficient ground for a psychological distinction. The following observations, however, will indicate further points of difference and deal more particularly with the main psychological differences noted in the 'projection' of the image.

IV. OBSERVATIONS CONCERNING THE PROJECTION OF THE IMAGE.

Certain fundamental differences are noted during the course of the projected image when compared with the normal after-image. The contrast is best seen in the foveal image obtained by fixating a bright filament for about 0·5 sec. An exposure is given, and the eyes are covered. The after-image attains maximum brightness and fades. The outlines become indistinct. If at this stage stimuli be given by allowing light to fall intermittently on the closed eyelids, the clear definite outline can be seen developing as it were out of the confusion and the image appears as if situated at a definite distance from the observer, in marked contrast to the indefinite spatial relationship of the fading after-image. The effect is even more definite if the eyes be opened and directed to a screen during stimulation. From the outset, then, the phenomena presented by the two types of image exhibit distinct differences, and whereas the after-image appears to be located mainly by the help of memory images of the position of the original object, the projected image gives to consciousness, at the instant that it is formed, a definite awareness of spatial relationship.

Other factors in connection with the projection of the image appear to be of interest. The projected image is readily placed in any desired direction by a movement of the eyes. A large image can be projected on a bright cloud, and by passing the hand rapidly before the eyes and at the same time making an effort of convergence, the image can be brought from this distance to within a few inches of the eyes, diminishing in size and giving by its apparent movement through space a weird appearance of objective reality, somewhat similar to that ascribed to hallucinatory images. The distance seems to be entirely determined by the extent of convergence, and does not seem to be dependent on the presence of external objects other than the extent to which they give light for stimulation and serve to determine accommodation.

Though the image readily follows voluntary movements of the eye carried out in the usual way by the eye muscles, the following experiments seem to indicate the presence of other factors.

If a bright filament, or a small bright disc be fixated with both eyes and then the image be projected and kept constant by intermittent stimulation at a favourable rate, the deflection of one eyeball by slight side pressure does not double the image.

If when the eyeball is displaced, the two images of a fixation cross on the screen are kept in a constant relative position by carefully maintaining the same degree of displacement, the projected image can be made to travel from one cross to the other, and in fact can be made to occupy any position whatever with regard to the two crosses.

There is, of course, the possibility that one of the images might have been inhibited owing to the displacement or to some other cause and only the image of the undeflected eye might have been formed. But the following experiment negatives this.

A bright filament placed at an angle of 45° from the vertical is illuminated and fixated with the right eye, the left being closed. Immediately afterwards, the left eye fixates the filament which has been turned through 90°. On stimulating the two eyes intermittently, the two images are seen, and form, combined, a cross: one stroke for the left eye and the other for the right eye. On displacing either eye the cross remains as perfect as before, showing that both eyes are forming the image and that there is neither inhibition nor displacement. In fact, both eyes may be quite considerably and actively displaced, making the spots on the screen move in quite a lively fashion, but the image of the cross remains fixed in whatever position the attention is directed.

To test this more severely, shutters were placed in front of each eye, so arranged that they opened and shut alternately and were completely under the control of the operator. When they opened and shut slowly, the image of the cross appeared to be rocking, as first one part and then the other formed in the image. The speed of the shutters was increased, till at the rate of about four exposures per second, the image ceased to rock and appeared as a stationary cross. If when this point was reached one eye was displaced so that a spot on the screen was doubled, the cross remained as perfect as before the displacement, showing that although the direction of the visual axis of one eye had changed, the direction of projection of the image for that eye had not changed.

At first sight this seems to be explained by the fact that the retina of the left eye has received a stimulus on the corresponding area to that of the retina of the right eye, so that no matter how disparately the eyes are turned, the two images will coincide owing to the fact that the intermittent light stimulates both corresponding areas.

In normal conditions the image always follows the voluntary movement of the eye; whereas when the eyeball is displaced by pressure, the image no longer follows the involuntarily produced movements of the eye, but occupies a position which seems to be located by the voluntarily determined direction of attention. That the retina has moved is shown by the displacement of the spot on the screen, and the fact that in one case the image moves with the eye when the movement is voluntary, whereas when the movement is involuntary no displacement takes place, requires explanation.

There seems to be some connection between this phenomenon and the facts noted in the case of those individuals who are subject to auto-kinetic sensations. In those persons who are liable to eye movement in the dark, an after-image produced by previous fixation of a bright object remains apparently fixed, while a bright point of light fixated in the dark appears to move. In the case of others the image and the point of light seem to move together. In view of these individual differences it is possible that in respect to the projected image similar divergences may be found both with regard to the movement of the image on fixation and to the behaviour of the image on displacement of the eyeball. So far, however, I have not had the opportunity to test this.

In connection with the foregoing experiments, it is interesting to recall the work of Binet concerning the experimental study of hallucinations induced in hypnotic patients. He states that in every case examined, the hallucinatory image is doubled by pressure on one eyeball. On the other hand, Dr Hack Tuke[1] and James[2] give several examples of patients who do not respond to this test and in whom there is no doubling of the hallucinatory image on displacement of one eyeball, indicating that for this type of hallucination there is probably a central origin, and that this disturbance is projected.

The projected image under favourable circumstances is of a well-marked hallucinatory character. When of considerable extent, it sometimes becomes so real that at times the whole aspect of consciousness changes in a manner very similar to that noted in a certain type of hallucination[1], and further, the agreement with regard to the doubling of the image on displacement of the eyeball gives additional grounds for hoping that with further enquiry an actual connection may be established between the phenomena of the projected image and certain types of hallucinations probably centrally excited.

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[1] *Brain*, XI.

[2] *Principles of Psychology*, II. 136.

V. SUGGESTED EXPLANATIONS.

The phenomena noted in the previous section are due to extremely complicated conditions and the following notes are added as suggestions of possibilities rather than as explanations.

At the outset, the action of intermittent stimulation calls for solution. Recent researches[2] have shown that a momentary light stimulus is sensed as a series of pulses of gradually decreasing intensity. It is evident that the intermittent play of light on the retina will be instrumental in the formation of a succession of such series. The observation that there is a certain favourable rate of stimulation at which the projected image attains its maximum intensity seems to point to the fact that when this stage is reached there is an advantageous summation or reinforcement of pulses which, following rapidly, give rise to a steady continuous image. The fact that maximum effect is reached in those paths which have been recently stimulated, thus differentiating these, seems to imply "facilitation" of impulses along these tracts. In addition experimental evidence was advanced[3] which shows that the image can be revived by stimulation of neighbouring areas, in the absence of direct stimulation of the initially excited retinal areas. This indicates the possibility of a 'spread' of impulses from the rest of the retina and in particular from those paths bordering those originally stimulated. Finally the repeated light stimulus may also produce further changes in the visual substances which have been acted upon previously, leading to an increase in the intensity of the impulses in those tracts initially stimulated by these changes.

The marked difference between the after-image and the projected image in the case of short exposures seems to involve important features. The after-image being dependent entirely on the continued activity in the visual tracts soon becomes blurred and indistinct, and it has been suggested that this change is due to the diffusion of substances formed during the initial stimulation and capable of exciting the retinal endings of the optic nerve. The marked way in which an indefinite after-image may be cleared up by intermittent stimulation through the closed eyelids and the fact that the projected image maintains its clearness of outline almost to its complete disappearance seem to show that the projected image depends mainly upon the re-excitation of the previously stimulated visual tracts by newly formed impulses, and to a lesser degree upon the re-excitation of the previously changed retinal substances. With longer exposures, and where the fixation is not accurately maintained, this difference between the two types of images is necessarily not so marked.

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[1] James, *Principles of Psychology*, II. 120.

[2] McDougall, this *Journal*, 1904, I. 93: "Momentary Stimulation of the Eye." See also Semon, *Die mnemischen Empfindungen*, 118.

[3] See Section III above.

When the image is projected on a screen the adjustment of the eye muscles necessary to bring the fixation point of the screen to a focus involves the activity of definite efferent neural paths. The voluntary innervation of the muscles of convergence and the accompanying accommodation gives a partial neural disposition. The intermittent stimulations bring about the completion of neural conditions and activity similar to what would have been present if an actual object situated at the distance for which the eyes have been adjusted, were observed. These conditions, before the arrival of the efferent impulse, are, so to speak, only in a state of preparedness. On stimulation, the image is therefore seen 'projected' about the point of regard.

When, by suitably directing the eyes, the image is made to move amongst external objects, other factors are introduced. The problem noted in the experiments on the displacement of the eyeball is even more complicated and appears to involve the subject's total relational experiences with reference to the external world. It has been shown that an intended but unsuccessful contraction of an eye muscle produces apparent changes in the relationship of the external world with regard to the subject. Under normal conditions the successful voluntary contraction of the eye muscles produces no change in the relation of the outer world towards the subject but is experienced as an alteration of the attitude of the person towards external objects. The point of regard is changed, and with it the position of any projected image changes. From previous considerations it will be seen that the displacement of the eyeball by pressure, though causing changes in optical convergence, does not alter the willed coordinated muscular contractions. Hence no displacement of the image occurs. With movements of the eyes, images of stationary objects pass across the retina and in some cases pass across the part stimulated in the initial exposure. During voluntary movement of the eye these changes of stimulation are interpreted as a movement of the image amongst the stationary objects. When the eyeball is displaced by pressure, similar retinal changes take place, but the passage of the images of external objects is experienced, as is generally the case in the absence of willed movement, as a movement of the external objects. The projected image in this case, however, having its foundation, so to speak, in conditions which are unaltered, owing to the unchanged volitional attitude of the individual in this respect, remains stationary. The external objects apparently move about the image.

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SIMULTANEOUS AND SUCCESSIVE ASSOCIATION.

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I. Introductory.
II. Experimental.
III. Results and Conclusions.
IV. Theoretical and Summary.

I. INTRODUCTORY.

THE present investigation was undertaken principally for the purpose of deciding by experiment, if possible, between the claims of *the rival theories of Simultaneous and Successive Association.* The theory of Simultaneous Association teaches that associations cannot be formed unless the mental experiences be simultaneous. To overcome the evident difficulty that, as we know from everyday life where experiences succeed each other in time, associations are formed, it is held that with the cessation of the actual mental experience the nervous excitation is not abruptly finished but continues, gradually dying away: that is to say, the mental experience is followed by a gradually diminishing subconscious phase, the akoluthic phase of Richard Semon[1]. The following mental experience coincides then in time with this akoluthic phase of the preceding mental experience, and it is due to this simultaneity that the association of the two experiences is formed. The simultaneity is a *sine qua non.* Associations of successive mental experiences then tend to form the limiting case to simultaneous associations.

The rival theory of successive association denies the possibility of associations between simultaneous experiences. In fact, it is an outcome of the theory that the range of our consciousness is not wide enough to attend to two things at once, to two objects of consciousness at the same time; but that it is constantly oscillating between two apparently simultaneous contents; and they thus are in reality successive experiences. Associations then between apparent simultaneous experiences form the limiting case to those of successive experiences.

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[1] Richard Semon, *Die Mneme*, 2te Aufl., 19.

Two other points which it appeared desirable to investigate at the same time were the influence of the closeness of connection between the members that were presented simultaneously, and the influence of certain attitudes of the mind upon the associations.

Altogether apart from the general fact of the unsuitability of nonsense syllables for any investigation on memory as shown by me in a previous paper[1], this unsuitability was here increased by the further fact that two syllables cannot be read simultaneously. I, therefore, chose colours and figures or shapes; and, in order to facilitate matters, I arranged them in pairs, each consisting of one colour and one figure. In doing this I not only overcame many difficulties that would have arisen through paucity of material, but I had also the advantage of having my subjects less troubled by the side-associations formed with the various members of the other pairs. Thus, for example, in a series of six pairs each consisting of a colour and a figure the subject during the examination could not be troubled by more than five side-associations, for, if a colour was shown, there were, beside the correct figure, only five others in the series, whilst if the series had consisted of colours only or figures only the number of possible side-associations would have been doubled, that is, side-associations might have been formed with any of the ten members constituting the other five pairs of the series.

II. EXPERIMENTAL.

I shall now proceed to give a description of the various groups of experiments concerned. These were groups 1, 2, 8 and 9. Groups 3 to 7 dealt with the direction of association and the results have been published in the paper mentioned above. I shall not here go into the technique of these experiments as this is fully described in that paper to which I must refer the reader.

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[1] "On Memory and the Direction of Associations," This *Journal*, 1913, v. 447 *seq.*

Group 1. *Experiments.*

The first purpose of the experiments was to investigate whether and how far the more or less close connection of the two members of a pair influences their association, and subgroups I were devised for this. These subgroups were three in number, viz., I α, I β, I γ. In I α the two members of a pair were the most intimately connected, *i.e.*, the colour was given to the figure itself, *e.g.*, a blue square, a green cross. etc. In I β the two members were less closely connected, the black figure being placed on a coloured background, *e.g.*, a black circle on a yellow field, a black oblong on a red field, etc. The connection was still looser in I γ where the black figure was placed by the side of the coloured field. In another subgroup, viz. II, the members of each pair were presented successively, it being intended that it should subserve by comparison with I γ the second object of this group, viz., to investigate the difference of associations by simultaneous and successive presentations respectively.

Each subgroup consisted of six series of six pairs of which the last four series only were counted, the first two series being regarded as preliminary experiments. The duration of the exposure of each pair in subgroups I α, β, γ and of each member in subgroup II was ·33 sec. The number of exposures of the series was the same in all subgroups; hence the total time for learning was equal in subgroups I α, β, γ and twice as long in II. Further details may be gathered from the following printed instructions given to each subject.

Instructions.

Sit down on the chair at such an easy distance from the apparatus that you can well see the window.

You will be presented in the window in rapid succession with a consecutive series, namely,

(I α) a coloured shape on a white ground.

(I β) a black shape on a coloured ground.

(I γ) a coloured field by the side of a black shape on a white ground.

(II) a coloured field followed by a black shape on a white ground or *vice versa*.

You are asked to learn these by attending to them in such an attitude of mind as to take in the sense-presentation without forming any verbal or other association than that given directly by the presentation. Thus, for instance, if the colour of the field is green, do not try to memorise it by calling it green, or if an octagon is shown you do not try to formulate its name, but simply take in the whole sense-presentation.

Do not try to remember the diagram and the colour by any artificial association (*e.g.*, if you were shown an arrow on a red field, "arrow draws blood—blood red," or a heart-shaped figure followed by a red field "heart love—love red," etc.). Avoid any such or similar associations.

After the series has been presented to you a certain number of times, the window will be closed. Cease at once to think about the experiment. If you find this impossible take up immediately the copy of *Punch* provided, and read it, giving yourself up entirely to your reading, and banish any thought of the experiment.

You will then be shown a colour or a diagram alone. Close your eyes. If a colour is given, look out for the diagram called up by it; if a diagram is given, for the colour it is associated with, or for the redintegration of the sense-presentation of which it formed part.

Avoid using any names for diagrams or colours, either aloud or mentally.

As soon as you have a definite result say "Now!" or as soon as you think that you cannot get a definite result, say "Nothing!" On saying the word "Now!" you will be shown a collection of diagrams or colours, as the case may be, and you are requested to point out, without naming, the diagram or colour called up and to state how they appear to be related. In pointing out the colour or diagram, state whether you feel "certain," "doubtful," "very doubtful," etc. If more than one colour or diagram are called up, point them all out and give details.

Record any further points revealed by introspection.

During the interval between the learning and the examinations avoid thinking about the experiment, and, if necessary, occupy yourself with reading the copy of *Punch* provided.

Special instructions for Series II. A coloured field is followed by a black diagram or *vice versa*. You are asked to learn these in pairs, each consisting of a coloured field and a diagram. To differentiate the pairs one from the other, they appear alternately on different sides of the window, *e.g.*, in the left-hand half of the window appears first a coloured field followed by a diagram, whilst the right-hand half is empty; then a coloured field followed by a diagram in the right-hand half of the window, whilst the left-hand half remains empty and so on.

As pointed out above, subgroup II (successive presentation of the members) had been intended for comparison with subgroup I γ (simultaneous presentation of the members). But a comparison of these subgroups seemed of little value for the following reason. In both subgroups the Müller memory apparatus worked at the same speed, *i.e.*, the contents of the window changed at the same equal intervals, and as in subgroup I γ the content was a pair whilst in subgroup II it was only one member of a pair; it is evident that it took twice as long to present a pair of the latter as it took to present a pair of the former. Hence to present the whole series also took twice as long for II as for I γ, and as all series were presented an equal number of times the total time for learning II was twice as long as that for learning I γ. It is, however, essential that the total time for learning should be equal in both cases and this could be accomplished in two ways: (1) giving half the number of exposures with successive pairs whilst their time of exposure is twice that of the simultaneous pairs (here the rate of the memory apparatus is the same in both series); (2) giving the same number of exposures with both series, and making the time of exposure of a successive pair the same as that of a simultaneous pair (here the rate of the memory apparatus has to be twice as fast for the successive as for the simultaneous series). The former condition was fulfilled in experiments of group 8 and the latter conditions in experiments of group 9.

The following diagrams show at a glance the relations of arrangements, of lengths of exposure and of total lengths of learning. A member of a pair is represented by an oblong, colours being shaded, say, horizontally and figures perpendicularly or *vice versa*. Time is represented from left to right. The height of an oblong may be said to represent the degree of consciousness, that is to say, if a single member of a pair is the sole content of consciousness for a given time, as in the successive series, it is represented by an oblong of a given height, as in subgroups 1 II, 8 *b*, and 9 *b*. Then when a whole pair is the sole content of consciousness, as in the simultaneous series, two oblongs of half that height are placed on top of one another, *vide* subgroups 1 I $\alpha\beta\gamma$, 8 *a*, and 9 *a*, so that the combined height of the pair is the same as the single height of a member in the successive series.

Groups 8 *and* 9. *Experiments.*

We have then two groups in which the total time of learning of both subgroups was of equal duration, viz.:

Group 8 (*a*) Simultaneous exposures: ·66 sec. each pair.
(*b*) Successive exposures: ·66 sec. each pair, *i.e.* ·33 sec. for each member.

N.B. The number of exposures per pair is the same in (*a*) and (*b*).

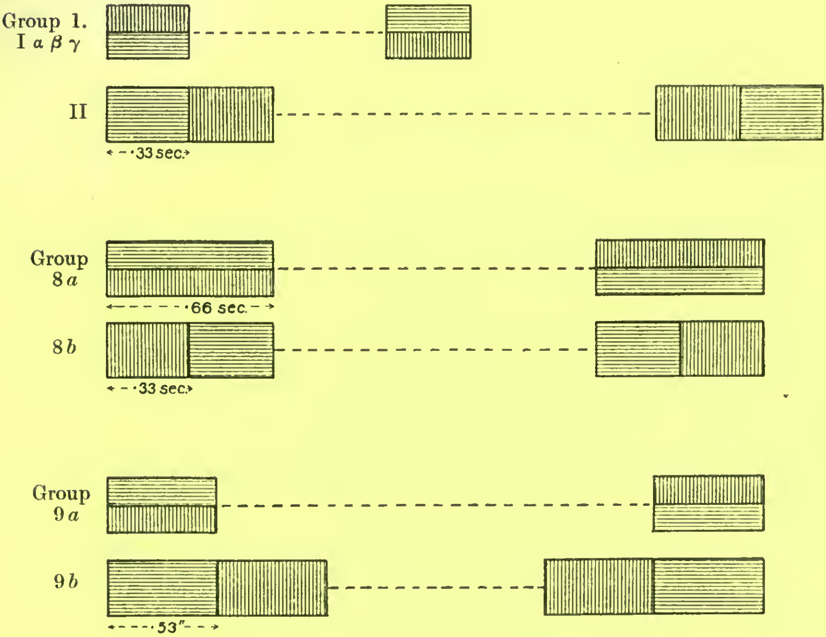

Fig. 1.

Group 9 (*a*) Simultaneous exposures: ·53 sec. each pair.
(*b*) Successive exposures: 1·06 sec. each pair, *i.e.* ·53 sec. each member.

N.B. In (*a*) the number of exposures per pair is twice that of the number of exposures per pair in (*b*).

In each subgroup 8 *a*, 8 *b*, 9 *a*, 9 *b*, there were four series of six pairs each, *i.e.*, 24 pairs, and as each member was given once as stimulus, therefore 48 answers were possible. The instructions were similar to those given above for group 1.

Group 2. Experiments.

There remains another group of experiments to be recorded, viz., group 2. Their main purpose was to put to a further test the *Successive Association Theory.* If all associations, as this theory asserts, be due to successive attending, then experiments where this is encouraged by definite instructions to that end ought to yield better results, however short the exposure, than experiments where any such tendency is counteracted by giving definite instructions to attend to the two members of a pair simultaneously. It appeared also desirable to examine what influence certain attitudes of mind had upon the forming of associations. The two attitudes that the subjects were asked to adopt were:

A. The Spectacular Attitude.
B. The Learning Attitude.

In the former attitude the attention was distributed, the subject was more or less passive, he was in a contemplative mood and made no particular effort to learn; he '*took in*' the pairs as they were presented. In the latter attitude the attention was concentrated upon the task, the subject was active and made the greatest effort to '*learn*' the pairs. In all cases the two members of the pairs were presented together, being placed side by side as in the simultaneous series 1 I γ, 8 *a* and 9 *a*, and in each attitude the subject was instructed to '*take in*' respectively, to '*learn*' either simultaneously or successively. The time of exposure of each pair was ·33 sec. with three subjects, with the fourth ·29 sec.—32 exposures of each series were given. The instructions were similar to those quoted above, but respecting the attitude the following was added:

For the purpose of these experiments you will be asked to adopt various mental attitudes, viz.:

A. The *Spectacular Attitude*: Look at the series, as it is presented to you, merely in a spectacular way, a passive or contemplative mood, making no particular effort to learn the pairs; disregard entirely the result of any subsequent examination; '*take in*' the pairs as they pass.

B. The *Learning Attitude*: Do your very best to learn the pairs. Whilst in the spectacular attitude the attention is distributed, you are now asked to make the greatest effort to concentrate your attention on each pair for the purpose of '*learning*' it.

You will be asked to '*take in*' (spectacular attitude) or to '*learn*' (learning attitude), as the case may be, either *simultaneously* or *successively*. This is to say: Attend to the members of each pair together for the former, or attend to the members of each pair in succession for the latter. In both cases it may be best to fixate the centre of the window, *i.e.*, a point between the colour and the diagram. This may even be advisable when attending to the members successively, as the time may be too short for eye-movements; allow the attention alone to move. It may perhaps be advisable to do this always in the same direction, say, from left to right, but it is left entirely to you to adopt any method you find easiest to carry out the above instructions.

III. Results and Conclusions.

The results of the group 1 experiments are given in the following Table 1:

TABLE 1. *Group* 1.

Subgroup		Simultaneous exposure of shape and colour. Duration of exposure of each pair ·33 sec.
Subgroup I α:	coloured shape	Simultaneous exposure of shape and colour. Duration of exposure of each pair ·33 sec.
,, I β:	black shape on coloured background	
,, I γ:	black shape by the side of coloured field	
,, II:	black shape and coloured field following one another	Successive exposure of members ·33 sec. each, *i.e.* ·66 sec. for each pair.

N.B. Total time of learning is twice as long for II as it is for I α, β, γ.

	Right answers											
	Median reaction-times				Actual scores (48 possible in each series)				Percentages of possible scores			
Subject	I α	I β	I γ	II	I α	I β	I γ	II	I α	I β	I γ	II
	sec.	sec.	sec.	sec.								
C	1·8	2·6	1·4	1·3	31	21½	11	31	64·6	44·8	22·9	64·6
F	3·6	4·6	3·7	2·9	45	33	24½	41½	93·7	68·8	51·0	86·4
L	1·0	1·2	1·2	1·0	48	41	35	44	100·0	85·4	72·9	91·7
R	2·0	2·2	1·8	1·6	42	38	23½	44	87·5	79·2	48·9	91·7
Averages	2·10	2·65	2·02	1·95	41·5	33·4	23·5	40·1	86·5 %	69·6 %	48·9 %	83·6 %
M. V.	·75	·97	·83	·65	5·2	6·1	6·2	4·6	10·9	12·8	13·0	9·5

On the left are given the Median Reaction-Times, in the middle the Actual Scores obtained out of a possible 48, and on the right these calculated in percentages.

Let us examine first the scores of the simultaneous exposures, viz., subgroups I α, I β, I γ. We find then that if the number of correct answers serves as a direct measure of the strength of the associations, this is nearly twice as strong when the members are as closely connected as in a coloured figure (I α), than if they are placed side by side (I γ), being 86·5 % against 48·9 %. The case where the figure is placed on a coloured background (I β) is nearly halfway between them, viz., 69·6 %.

As to the reaction-times it is remarkable that they do not indicate any difference in the readiness of the associations, for they are all about equal, being 2·10, 2·65, 2·02 sec. for I α, I β and I γ respectively, whilst previous work on associations would lead one to expect a shorter reaction-time for a stronger association, *i.e.*, the reaction-time of I α to be shorter than that of I β and this shorter than that of I γ.

Reverting to the numbers of right answers we find that the predominance of I α over I β and of this over I γ obtains with all four subjects, there is not a single exception. The number of right answers decreases everywhere with the closeness of the connection between the two members of a pair. In I α the members are connected the most closely, being blended into a coloured shape. The connection is less close in I β, where the black figure rests on a coloured background, and still less in I γ where the black figure and the coloured field are placed side by side. We may therefore state the law:

The more the members of a group are apperceived as a whole, the stronger their association with one another.

As to subgroup II this, as stated above, had been intended for comparison with subgroup I γ. However, a comparison is really not possible, for although we have the same number of exposures of each pair in both subgroups *each member* of a pair of subgroup II had the same length of exposure as *each pair* in subgroup I γ, hence each pair of the former was exposed twice as long as each pair of the latter and consequently the whole time of learning a series of subgroup II was twice as long as the learning of a series of subgroup I γ. As it is, with the same number of exposures but twice the time for learning, the successive series of subgroup II show 83·6 % of correct answers against 48·9 % of the simultaneous series of subgroup I γ. It is, however, interesting to observe that although the time of learning is twice as long the result does not even come up to that of subgroup I α of the coloured shapes.

To carry out the main purpose of these investigations, viz., to test the claims of the rival theories of simultaneous and successive associations, it became necessary to devise fresh sets of experiments, viz., groups 8 and 9 which have been described above. The results are given in Tables 2 and 3.

TABLE 2. *Group* 8.

Subgroup (*a*). Simultaneous exposures, ·66 sec. each pair.

„ (*b*). Successive exposures, ·66 sec. each pair (*i.e.* ·33 sec. each member).

N.B. The number of exposures is the same in (*a*) and (*b*), *i.e.* the total time of learning is the same in both.

	Right answers					
	Median reaction-times		Actual scores (48 possible in each subgroup)		Percentage of possible scores	
Subject	*a*	*b*	*a*	*b*	*a*	*b*
	sec.	sec.				
B	2·6	1·6	20½	6	42·7	12·5
C	1·6	1·7	42½	38	88·5	79·2
F	2·9	2·9	46	42	95·8	87·5
H	2·3	2·0	40	36	83·3	75·0
J	1·7	2·3	31½	45	65·6	93·7
P	3·2	2·9	40	35	83·3	72·9
St	2·2	2·0	30	32½	62·5	67·7
W	2·0	2·8	44	41	91·7	85·4
Averages	2·3	2·3	36·8	34·4	76·7	71·7
M. V.	·44	·39	7·1	7·7	14·8	15·9

The reaction-times of group 8 are the same both for (*a*) and (*b*), viz., 2·3 sec., whilst the former series, viz., (*a*) the simultaneous one, gives 76·7 % correct answers against 71·7 % for (*b*) the successive series.

In group 9 the reaction-time for (*b*) is somewhat longer than for (*a*), viz., 2·6 sec. against 2·3 sec., thus corresponding with the scores, viz. 69·1 % for the former against 73·9 % for the latter, *i.e.*, the series that shows a slightly weaker association also shows a slightly longer reaction time. It may be noted that the percentage of right answers in group 9 is somewhat lower both for (*a*) and (*b*) than in group 8. This is doubtless due to the fact that in group 9 the total time of learning was shorter than in group 8, since the time of each exposure was ·53 sec. in group 9 and ·66 sec. in group 8.

Now comparing the experiments of group 8 with those of group 9 we are led to the following considerations. The experiments of 8 *a* and 9 *a* are identical with the exception of the duration of the exposures.

TABLE 3. *Group* 9.

Subgroup (*a*). Simultaneous exposures, ·53 sec. each pair.
,, (*b*). Successive exposures, 1·06 sec. each pair, *i.e.* ·53 sec. each member.

N.B. Number of exposures of each pair in (*a*) twice that in (*b*), *i.e.* the total time of learning the same in both.

	Right answers					
	Median reaction-times		Actual scores (48 possible in each subgroup)		Percentage of possible scores	
Subject	*a*	*b*	*a*	*b*	*a*	*b*
	sec.	sec.				
B	1·8	2·7	16	18	33·3	37·5
C	2·0	1·9	41½	35½	86·4	73·9
F	2·6	2·8	45	41	93·7	85·4
H	2·0	2·0	38	30	79·2	62·5
J	2·0	2·7	28	22	58·3	45·8
P	3·2	3·0	37½	41½	78·1	86·4
St	2·6	2·2	38	35½	79·2	73·9
W	2·1	3·1	40	42	83·3	87·5
Averages	2·3	2·6	35·5	33·2	73·9	69·1
M. V.	·39	·38	6·75	7·4	14·1	15·4

But whilst this difference of duration of exposure remains constant in the experiments of 8 *b* and 9 *b* the number of exposures in these two subgroups differs, being twice as great in 8 *b* as in 9 *b*. Hence, if the theory of successive association be correct, *i.e.*, if the formation of association be due to succession *qua* succession, the results of 8 *b* as compared with those of 8 *a* should be greater than those of 9 *b* as compared with those of 9 *a*, in other words we should get

$$\frac{8a}{8b} < \frac{9a}{9b}.$$

But from Tables 2 and 3 we obtain

$$\frac{8a}{8b} = \frac{76{\cdot}7}{71{\cdot}7} = 1{\cdot}07,$$

and

$$\frac{9a}{9b} = \frac{73{\cdot}9}{69{\cdot}1} = 1{\cdot}07,$$

i.e., they are equal. We are therefore forced to conclude that *with colours and diagrams*, i.e., *with the psychological memory*[1], *the theory that the formation of associations is due to the succession of experiences* qua *succession is not correct.* From which it follows, since the rival theory seems the only alternative, that *with respect to the psychological memory all associations are due to simultaneity, either simultaneity of the experiences, or simultaneity of the succeeding experience with the akoluthic phase of the preceding experience.*

This question may, however, as already indicated above, be approached from another side. If we expose simultaneously a pair consisting, say, of a colour and a diagram and in one set of experiments instruct the subject to attend to the two members simultaneously whilst in another set the subject is ordered to attend to the members successively, then, if the theory of successive association be correct, the experiments with successive attending ought to give the better results. For here the natural tendency of the subject would be reinforced, whilst the ordered simultaneous attending would have the effect of counteracting such natural tendency. In the preliminary experiments the time of exposure was reduced until it was difficult to attend successively, whilst it was still possible to attend to the two members simultaneously. Thus introspection already indicated the result to be expected. The threshold was found to be at about ·33 sec. for most subjects, but with one this length of exposure had to be reduced to ·29 sec.

The experiments of group 2 were devised for this purpose and they also subserved another one, viz., that of ascertaining the influence of special attitudes of mind, as explained above on p. 440. Table 4 gives the results.

The reaction-times are decidedly longer for the spectacular than for the learning attitude and whilst in the learning attitude they are nearly equal for simultaneous and successive attending, this latter shows a marked longer reaction-time than the former in the spectacular attitude. It will also be noted that with two of the four subjects, viz., F and Sp, the reaction in the spectacular attitude is longer for successive attending, whilst with the same two subjects it is longer for simultaneous attending in the learning attitude. With subject C these conditions are reversed and with the fourth subject J the reaction-time for successive attending is longer in both attitudes.

It may here be mentioned that all these experiments, but especially those of successive attending, were exceedingly trying and most fatiguing, and it happened repeatedly with subjects F and J, as the protocols show, that perseveration interfered. When during an experiment one pair had made room for another pair in the window of the apparatus, the memory-image of the pair just gone perseverated for some short time to the detriment of the succeeding pair, and during this perseveration it was possible to attend to the two members successively. Had

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[1] *Vide* my paper "On Memory and the Direction of Associations," This *Journal*, 1913, v. 465.

TABLE 4. *Group* 2.

Subgroup *A*. Spectacular attitude. (*a*) Simultaneous attending.
(*b*) Successive attending.
,, *B*. Learning attitude. (*a*) Simultaneous attending.
(*b*) Successive attending.
Exposure ·33 sec. each pair[1].

Subject	Right answers											
	Median reaction-times				Actual scores (48 poss. in each series)				Percentages			
	A		*B*		*A*		*B*		*A*		*B*	
	a	*b*	*a*	*b*	*a*	*b*	*a*	*b*	*a*	*b*	*a*	*b*
	sec.	sec.	sec.	sec.								
C	2·0	1·9	1·3	2·2	25½	3	40	14	53·2	6·3	83·3	29·2
F	6·4	8·8	3·9	2·8	26	9	43	40[2]	54·1	18·8	89·6	83·3[2]
J[1]	2·9	3·4	2·4	3·7	13½	9[2]	20	16½[2]	28·1	18·8[2]	41·7	34·2[2]
Sp	3·2	4·0	3·1	1·2	13	1½	9½[3] in 36	3[3] in 36	27·1	3·1	26·4	8·3
Averages	3·6	4·5	2·7	2·5	19·5	5·6	28·9	18·6	40·6	11·7	60·2	38·7
M. V.	1·4	2·1	·8	·8	6·2	3·4	12·6	10·7	13·0	7·1	26·2	22·3

[1] For subject J the time of exposure of each pair was ·29 sec. only, for it had been found that an exposure of ·33 sec. was well above his threshold for attending successively to the two members of each pair.

[2] Results too high owing to several cases of perseveration.

[3] Subject Sp could not complete the full number of sittings. In calculating the averages the figures were corrected for 48 possible right answers.

these two subjects been able to carry out rigorously the instructions given them, there is no doubt that the results would have been still more decisive, *i.e.*, the numbers of right answers obtained here would have been still lower. We find nevertheless that the results of simultaneous attending are very much better for both the spectacular and the learning attitudes than those of successive attending. All subjects were trained observers and they stated unanimously that it was mostly quite impossible to attend successively to the two members of a pair during the short exposures of ·33 sec. and ·29 sec. respectively, whereas they could be attended to simultaneously with comparative ease.

Turning then to the scores, we find, as was to be expected, that these are respectively decidedly larger for the learning attitude than for the spectacular attitude, and in each attitude also the scores for the simultaneous attending are much larger than those for the successive attending, being:

	Simultaneous attending	Successive attending
Spectacular attitude	40·6 %	11·7 %
Learning attitude	60·2 %	38·7 %

We have therefore the fact that the set of experiments where an oscillation of attention was encouraged produces much feebler associations than those experiments where such oscillation was prohibited; and this confirms the conclusions derived from the experiments of groups 8 and 9, as stated on pp. 443, 444.

To the possible significance of the better results in the learning attitude as compared with the spectacular attitude we shall revert later (p. 450).

It may here be convenient to point to some apparent individual differences that seem to be indicated by the results of groups 8 and 9. If we construct a table putting against the subject's initial in each group that mode of learning (*a* = simultaneous, *b* = successive) in which the subject appeared to excel, we get

Group	8	9
Subject B	*a*	*b*
C	*a*	*a*
F	*a*	*a*
H	*a*	*a*
J	*b*	*a*
P	*a*	*b*
St	*b*	*a*
W	*a*	*b*

That is, three subjects (C, F, H) excel in simultaneous learning in both groups, the other five subjects excel in simultaneous learning in only one group, viz., B, P and W in group 9, and J and St in group 8. If we go, however, into the figures (number of scores) in order to see how much this excellence amounts to, we find that these figures leave a decided balance in favour of *a*, the simultaneous exposure. We may therefore conclude that *under the given experimental conditions, viz.,* *with the psychological memory, the simultaneous experience is more favourable for the learning of pairs than the successive experience.*

Having finished the examination of the results of our experiments we may attempt an analysis of the wrong answers recorded. All the experiments of the various groups consisted of series of six pairs which we may represent by *A a*, *B b*, *C c*, *D d*, etc., where a capital letter stands for, say, a diagram and a small letter for a colour. Now suppose in the course of the examination *B* be given as stimulus, *b* would be right and any other answer wrong. The wrong answers obtained were classified according to their relative position to the stimulus. If the

TABLE 5.

Experiment	Subject	Preceding	Succeeding	Others	Experiment	Subject	Preceding	Succeeding	Others
Group 1. I *a*	C	2	1	5	Group 8 *a*	B	4	2	11
	F	2	–	1		C	–	–	3
	L	–	–	–		F	1	–	1
	R	–	–	2		H	1	1	–
β	C	5	3	8		J	4	2½	5
	F	3	1	10		P	2	–	–
	L	–	–	1		St	4	6	7
	R	–	1	2		W	–	1	2
γ	C	1	2	7	*b*	B	3	4	16
	F	8	5	12		C	1	1	5
	L	–	–	3		F	–	–	5
	R	2	–	14		H	–	–	3
II	C	–	1	10		J	–	1	–
	F	3	2	2		P	1	2	–
	L	–	–	–		St	2	2	7
	R	1	–	3		W	–	–	2
„ 2. A *a*	C	4	5	5	„ 9 *a*	B	5	1	16
	F	4	4	10		C	–	–	2
	J	1	4	9		F	1	–	2
	Sp	4	3	2		H	2	–	4
b	C	2	2	1		J	2	2	4
	F	6	6	19		P	–	–	3
	J	3	2	10		St	1½	3½	2
	Sp	1	2	1		W	1	1	–
B *a*	C	3	2	4	*b*	B	2	1	11
	F	2	1	1		C	2	3	5
	J	3	3	9		F	3	2	2
	Sp	2	1	1		H	2	2	6
b	C	4	4	5		J	6	6	9
	F	1	–	4		P	–	–	–
	J	5	2	7		St	5	2	5½
	Sp	–	1	1		W	2	2	–
					Totals....		129½	106	307½

reaction to *B* was *a*, then the answer was classed as "*preceding*," if *c*, as "*succeeding*," any other answer as "*others*." In this manner we shall be enabled to see whether any stronger associations exist between the members of adjacent pairs than between members of more distant pairs. Table 5 gives the result of this analysis.

The ratios are

preceding : succeeding : others :: 1·2 : 1 : 2·9
or 23·5 % : 19·6 % : 56·9 %.

The wrong answers of the four groups of experiments were pooled, for all the experiments agree in that the pairs consist of a colour and a diagram each and they follow one another immediately. If associations were established between adjacent pairs, this would be due to the constant conditions and independent of the variable conditions of these experiments, as described above. On the other hand, it is admitted that taking the numbers of the wrong answers as they stand, without corrections, is open to objection. It may, for example, be argued that there are a great number of cases where no answer at all was given and that these ought to be regarded as potential answers and distributed among the actual right and wrong answers. Treated in this manner the ratios obtained are:

preceding : succeeding : others :: 1·1 : 1 : 2·4
or 24·4 % : 22·2 % : 53·4 %.

Again it may be argued that the result of each subject ought to be expressed separately in proportions and their average taken. If this be done we get

preceding : succeeding : others :: 1·3 : 1 : 3·0
or 24·5 % : 18·9 % : 56·6 %.

The small differences between the results obtained by the three methods show plainly that controversy is unnecessary. It will be seen from any of the ratios that there is no decided prevalence of the adjacent pairs. This is to say that the distribution of the wrong answers is approximately according to chance. For five wrong answers are possible to a stimulus and of these one belongs to the "preceding," one to the "succeeding," and three to "others," *i.e.*, according to mere chance the ratio ought to be

preceding : succeeding : others :: 1 : 1 : 3
or 20 % : 20 % : 60 %.

We may therefore conclude that *under the given experimental conditions, viz., learning in pairs, with psychological memory, no cross-associations between members of adjacent pairs are formed.*

IV. THEORETICAL AND SUMMARY.

In a previous paper above referred to[1] I drew a distinction, as far as the nervous system is concerned, between a higher and a lower memory, the psychological and the physiological memories. An essential difference there pointed out consisted in the experimentally established fact, that in the psychological memory the direction of association, as revealed in ekphory[2], is of equal value both forward and backward, whilst in the physiological memory it is forward only. That is to say, given a sequence of experiences......*k*, *l*, *m*,......, *l* being given again at some future time, then, with the psychological memory, *k* is as likely to be ekphored as *m*, whilst with the physiological memory *m* only is likely to be ekphored, and never *k*. Possibly, however, there is still another difference, for whilst with the physiological memory, similarly to the old associationist doctrine, a mechanical sequence might suffice to establish association (this is an assumption, and is not proved), my experiments tend to show that this is not the case with the psychological memory. We see from group 2, the special attitudes experiments (Table 4, p. 446), that the learning attitude with successive attending (B *b*) produced in every individual case better results than the same attending in the spectacular attitude (A *b*). This may in some measure be due to the possibility that the conditions of the spectacular attitude approach *more*, while those of the learning attitude approach *less*, to those of mechanical sequence.

The following considerations point in the same direction. If a mere sequence sufficed to establish associations (in psychological memory), these should be as powerful, or nearly so, between the second member of a pair and the first member of the succeeding pair as between the members of a pair itself. Thus if *A a*, *B b*, *C c* are succeeding pairs (capitals standing for diagrams and small letters for colours), then the associations between *a* and *B*, *b* and *C*, etc., should be almost as strong as between *A* and *a*, *B* and *b*, etc. This, however, as has been shown above (p. 449), is not the case.

We have therefore some right to assume that a higher psychical function is necessary in grouping, in uniting the members into pairs, and introspection appears to show that this is accomplished by a corresponding setting of the mind. Not, as might be thought, if, say, *A* is followed by *a*, these are subsequently grouped together as a pair, and so on, but rather the idea of "*a pair*" is given first and *A a* etc. are then, as it were, received into it. To put this diagrammatically, the active formation of pairs appears to be

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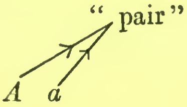
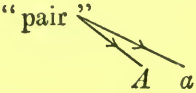
[1] *Loc. cit.* 462–465.

[2] Richard Semon, *l.c.* 26.

not



but rather



(time being represented from left to right), *i.e.*, "pair" precedes "*A a* = pair."

The physiological basis of association and of this integrating function of learning in pairs may be conceived in the following manner. Two or more groups of neurones (concerned in psychological memory) that are excited, *i.e.*, either during the simultaneous mental experience or during the akoluthic phase of a just passed mental experience and a present mental experience, become associated by lowering the resistance of the association-fibres, connecting them, so that when on a future occasion one of the groups is excited again this excitation may pass along the association-paths and communicate itself to the other groups of neurones. It does not matter in which direction the excitation passed on the first occasion—the formation of the engram[1],—there is no law of forward conduction in the psychological memory as there is in the physiological memory. As to the learning in pairs this integrating function might be conceived to modify the central occurrence as follows:—

The excitation of groups of neurones corresponding to the idea of "pair" and to the determination to apperceive the expected experiences as such, reinforces the excitation of the groups of neurones constituting the physiological basis of these experiences and facilitates the association-paths between them, inhibiting at the same time the transference of any energy to the groups of neurones that are being excited by the stimuli corresponding to the succeeding pair, and thus blocking any paths that might have served as association-paths between the first set of groups and the second set.

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[1] Richard Semon, *l.c.*, 22.

To conclude we will summarise the experimental results of this investigation which *refer to the psychological memory only.*

(1) The more the members of a group are apperceived as a whole the stronger their association with one another.

(2) The theory that the formation of associations is due to the succession of experiences *qua* succession is not correct.

(3) All associations are due to simultaneity, either simultaneity of the experiences, or simultaneity of the succeeding experience with the akoluthic phase of the preceding experience.

(4) The simultaneous experience is more favourable for the learning of pairs than the successive experience.

(5) In learning pairs no cross-associations between members of adjacent pairs are formed.

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FACTORS IN THE MENTAL PROCESSES OF SCHOOL CHILDREN[1].

I. VISUAL AND AUDITORY IMAGERY.

By N. CAREY.

(From the Psychological Laboratory, University College, University of London.)

I. Historical and Introductory.
  1. *Early views concerning the function of images.*
  2. *Results of indirect methods of investigating the function of imagery.*
    (*a*) *Galton's work.*
    (*b*) *Researches supporting the doctrine of 'imageless thought.'*
    (*c*) *Researches in which imagery has been found useful in mental operations.*
    (*d*) *Vicarious functioning of imagery.*
  3. *Results of direct methods of investigation.*
  4. *Imagery and educational practice. Need for further investigation.*

II. Experimental.
  1. *Verbal memory tests.*
  2. *Tests of sensory discrimination.*
  3. *Tests of sensory memory.*
  4. *Tests of general ability.*
  5. *Tests designed to involve the use of imagery.*
  6. *Introspections concerning visual and auditory imagery.*
  7. *Estimates of scholastic ability according to the results of school examinations.*
  8. *Teachers' lists.*

III. Reliability.
  1. *Procedure.*
  2. *Small individual variations in ability tested.*
  3. *Defects of method.*
  4. *Influence of attention.*
  5. *Mass v. individual tests.*

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[1] Thesis approved for the degree of Doctor of Science in the University of London.

IV. Examination of the validity of the introspections.
  1. *Criticism.*
  2. *Criteria.*
  3. *Distraction experiments.*
  4. *Agreement between different tests of image type.*
  5. *Correlation of one set of introspections with another made six months later.*
  6. *Comparison of children's introspections with those of trained psychologists.*

V. Results.
  1. *Correlation between different classes of imagery.*
  2. *Imagery and sensory discrimination and memory.*
  3. *Imagery and verbal memory.*
  4. *Imagery and the tests designed to correlate with it.*
  5. *Imagery and the school subjects.*
  6. *Imagery and the teachers' estimates.*
  7. *Imagery and the general ability tests.*

VI. Summary.

I. HISTORICAL.

1. FROM the very beginning of psychology it was commonly taken for granted that the memory of an experience consisted in the imaginal reproduction of that experience, the imagery being of the same nature as the actual experience but of weakened intensity. Thus memory is defined by Aristotle[1] as "the permanent possession of a sensuous picture as a copy which represents the object of which it is a picture." He also writes that "without the aid of sense-perception we never come to learn or understand anything; and whenever we consider something in the mind, we must at the same time contemplate some picture of the imagination, for the pictures of the imagination correspond to the impressions of the senses, except that the former are without material embodiment[2]."

Berkeley also assumes the presence of imagery in all our thinking. In abstract thought, which cannot be adequately reproduced by mental imagery, he denies the existence, or the possibility of the existence, of abstract 'ideas,' but maintains that in such cases we have particular 'ideas' which represent or stand for all other particulars of the same kind. The imaginal symbol is for the most part present in consciousness without its signification, as when one solves a problem in algebra, without being aware of the particular quantity for which each letter stands[1].

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[1] Aris. *Psych.*, trans. by F. Wallace, Introd. xciv.

[2] *Ibid.*, 173.

The same assumptions still exist in many of the present-day text-books. Thus, it is maintained that the processes of recognition and comparison cannot be accomplished without the aid of imagery. Recognition is described as the reproduction of the image of an object experienced in the past, with which the present object is identified. The process of comparison of a present object with one not present is said to be the reading off of the likenesses and differences between the present object and the revived image of the object not present. Perception, too, is often described as the psychical synthesis of present sensations excited by the perceived object, with images of sensations of that object previously experienced. Upon this second factor of the synthesis is said to depend the ability to learn by experience.

On the subject of creative imagination, Ribot says: "Man is able to create for two principal reasons. The first, motor in nature, is found in the action of his needs, appetites, tendencies, desires. The second is the possibility of a spontaneous revival of images that become grouped in new combinations[2]."

Peillaube, in his book on images, ascribes to them an important function in almost every mental process. Betts, in *The Mind and its Education*, writes (p. 99): "Binet well says that the man who has not every type of imagery almost equally well developed is only a fraction of a man."

The necessity of the presence of imagery in most of our mental processes has also been maintained by many physiological psychologists. It is assumed that the image results from the excitation of the same nerve centre as the corresponding sensation, and that the image is to memory what sensation is to perception.

2. (*a*) Even after more exact methods of investigation were used, the same idea dominated psychology. Thus Galton[3], after inquiring into the nature of mental imagery, concluded without further inquiry that "there can be no doubt as to the utility of the visualising faculty when it is duly subordinated to the higher intellectual operations." And again: "The best workmen are those who visualise the whole of what they propose to do, before they take a tool in their hands."

(*b*) With Külpe, Bühler, Ach, Watt, Messer, Koffka and others arose a school of more accurate methods of introspection. The subjects were given a specific task to perform and then required to analyse the processes immediately after. The results went to show that thought does not consist wholly of images or even nearly so. This has been corroborated by Dr Aveling, who after experimenting and obtaining a large number of introspections, concludes that "images are not necessary as content for thought[1]." Other well-known psychologists also assert that the thought processes can be carried on without the aid of imagery of any kind. For example, Prof. Woodworth obtained a number of introspections concerning the condition of mind preceding certain simple movements. In discussing the results he says that "the thought may be clothed in sensorial images, rags and tatters or gorgeous raiment, but these are often, after all, only clothes, and a naked thought can perfectly well perform its function of starting the motor machinery in action and determining the point and object of its application[2]." In another place he says that "probably all thinking originates in terms of meaning, but that some persons have more excitable sensorial processes and so get images[3]."

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[1] *Principles of Human Knowledge* (Everyman edition), xii, 100, and xix, 107.

[2] *Essay on the Creative Imagination*, 313.

[3] *Inquiries into Human Faculty*, 78.

T. V. Moore, in his *Study of the Process of Abstraction*, concludes from the introspections which he obtained: "Assured recognition is not dependent upon perfect perception. For assured recognition can exist when perception is so imperfect that no mental image whatever exists. The theory that recognition is brought about only by a comparison of the present sensation with a revived mental image is entirely refuted. Certain recognition can take place without the formation of any mental image whatever of the thing that is recognised."

(*c*) Other researches have been made, the results of which are in opposition to those given above. For example, Starch[4] found that persons practised in certain arithmetical mental tests, showed improvement in other arithmetical mental operations in which they were not practised. From the introspections he concludes that one cause of the transfer of training was the acquisition of the ability to visualise arithmetical operations.

Titchener says too: "I have turned round, time and time again, upon consciousnesses like doubt, hesitation, belief, assent, trying to remember, having a thing on my tongue's tip, and I have not been able to discover the imageless processes[5]."

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[1] *Consciousness of the Universal,* Macmillan & Co.

[2] *The Cause of a Voluntary Movement* (Garman Com. Vol. 1906), 392.

[3] *J. of Philos., Psychol. and Sci. Meth.* 1906, 708.

[4] "Transfer of Training in Mental Operations," *J. of Educ. Psychol.,* June, 1911.

[5] *Experimental Psychology of the Thought Processes,* 182.

(*d*) Standing, in a sense, between these two sets of results, are the results of other researches in which it has been found that the imagery in the reproduction is not in the same terms as the original experience. Thus Bentley[1] carried out some tests in order to determine the nature of the memory image; and from the introspections he concludes that memory is not a weakened reproduction of a previous experience, that reproduction is very schematic and that other aids are more important in remembering. Whipple's experiments[2] on the discrimination of tones indicate that an auditory image is not necessary in reproducing, recognising or comparing tones. One reason that Binet gives in his article "La Pensée sans Images[3]," for supporting the doctrine of imageless thought, is that according to the observations of Armande and Marguerite, the imagery during a mental operation is often quite irrelevant. H. H. Hollingworth, on the other hand, does not consider that irrelevancy is any argument against the usefulness of imagery and that such imagery may function vicariously. He says: "Just as in logic a relation presupposes substantives which are related, so in psychology, the consciousness of relation implies the presence of sense content of some kind or other. Association areas, the activities of which may underlie the feeling of relation, do not initiate their own activity. Just as a relation involves related things, so association tracts function only as connecting lines between sensory or sensory and motor centres. The curious thing, however, is that any pair of sensory centres will suffice, no matter how irrelevant or derived they may be....The sensory material serves merely as a vehicle, the instrument of the process, and any vehicle will do[4]."

3. By Thorndike, Betts and Ruger, we are furnished with a new and direct method of attacking the problem. Thus, Thorndike[5] in some experiments on University students tried to determine directly the relation between the clearness of visual images and their efficiency of function. The students were arranged in ten groups according to the clearness of their visual images. Then questions were set as to the visual appearance of various objects. Thorndike concludes that his

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[1] "The Memory Image and its Qualitative Fidelity," *Amer. J. of Psychol.* 1899.

[2] "Memory Image and Process of Judgment in discrimination of Clangs and Tones," *Amer. J. of Psychol.* 1901.

[3] *Rev. Phil.* 1903.

[4] "Vicarious Functioning of Irrelevant Imagery," *J. of Philos., Psychol. and Sci. Meth.* 1911, 692.

[5] "Function of Images," *J. of Philos., Psychol. and Sci. Meth.* IV. No. 12.

results go to show that a good visualiser does not necessarily gain more knowledge of the aspect of things than a poor one.

In some experiments on mental multiplication he also found that "there is apparently a zero or a slight negative relationship with the vividness and fidelity of the visual images of the numbers, partial products, etc. The proportions of those of strong and weak visual images were closely the same in those improving much and those improving little. Of the few cases who reported increase or decrease in the strength of the visual images of the numbers, during the course of practice those who reported a decrease improved somewhat more[1]."

The most extensive direct investigation into the function of images is that of Betts, whose earlier opinion on imagery is given on p. 455. He deals with both voluntary and spontaneous imagery. With reference to the latter he obtained introspections as to its function in discrimination, logical thinking, the solving of spatial problems and the appreciation of literature[2]. The results show that the introspected images have little if any correspondence with the powers of adults in these mental processes. Another direct investigation of the usefulness of images is that of Ruger. He graded his subjects according to the clearness of their visual images, and then compared this grading with their powers of solving mechanical puzzles. Here, if in no other case, one would expect images to be useful. But Ruger says: "The relative independence of analysis and imagery is shown by the double fact that, on the one hand, the mere presence of images directly related to the solution of the problem in hand was found to be of no avail unless the act of analysis were performed, and on the other hand, that previous experience seemed to be effective in determining a perceptual analysis in which no trace of imagery could be found[3]." And again: "The mere presence of imagery, although vivid and of closely related puzzles, was no guarantee of its efficiency. Very often attention rested on some superficial point of similarity, and progress toward solution seemed to be delayed instead of hastened." The subject who made the best records in this research stated that he was entirely without visual imagery.

Of these functional experiments there has been a good deal of criticism. For example, various critics of Betts' work have accused him of basing his conclusions on amateurish introspections, and doubts have been raised as to whether the function of the image can be determined by such a method. But hitherto, at any rate, there seems to have been no direct and experimental evidence whatever.

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[1] "The Effect of Practice in the case of a purely Intellectual Operation," *Amer. J. of Psychol.* 1908.

[2] *Distribution and Function of Mental Imagery,* 1909.

[3] "The Psychology of Efficiency," *Arch. of Psychol.* 1910.

4. On the whole, the results of experiment go to show that the function of imagery in adults has been greatly over-estimated. But it is still generally held, that it is useful in the mental processes of children. Even Betts[1] says: "that children should employ much more imagery than adults in their thinking, is wholly in accord with the mode of the mind's development." The object of this research is to test the validity of such an assumption[2]. That the question is an important one is evident when one considers how much present-day educational practice is based upon its truth. For example, the following is an extract from a paper on psychological analysis and educational method in spelling, read at the Psychological Section of the British Association meeting 1913: "In the total word-complex there are the visual and writing-motor elements forming the written symbol, and the auditory and speech-motor elements of the spoken symbol. The visual and auditory elements may be either perception or imagery; the motor elements either actual movement or imagery." And again, in another paper on the same subject: "The good speller normally achieves success through constant practice in reading and writing, whereby correct mental images, visual, auditory and motor, are obtained largely on the margin of attention." In both these cases the function of imagery is taken for granted. The following extract (and examples could easily be multiplied from other books) affords further evidence of this fact: "As bricks could not be made without straw, so constructive imagination cannot act without mental images. The constructive imagination is called into play by stories, reading, arithmetic, geography, and history, providing they are taught as they should be, and by all directions such as are given in physical exercises. The proper understanding of lessons, and the development of accurate constructive imagination, cannot be brought about by allowing the pupil to perceive every object and combination every time, but by having them partly imaged and partly shown, then imaged by the help of simple pictures, diagrams or gestures, and finally by means of words only[3]."

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[1] *Loc. cit.*, 93.

[2] The research was suggested to me by Prof. Spearman, and owes its existence to his never-failing guidance and encouragement.

[3] E. H. Kirkpatrick, *Fundamentals of Child Study*, 263.

The following extract serves to show the present need of investigation of the function of images from a pedagogical point of view: "A relatively abstract treatment of imagery has hitherto prevailed: psychologists have explored the 'properties' of images much as a chemist may explore the properties of an element. For pedagogical purposes we now have more need of a natural history of such processes as reading, writing, calculating, etc., a treatment which regards them as specific psychological structures, determines the possible difference of type and investigates the part played by imagery in their development.... What is the relation of imagery to the process of reasoning? How do the teacher's own peculiarities of imagery affect his exposition and his pupil's assimilation? What is the importance of the alleged connection between imagery and emotional vividness? (This is a question with an obvious bearing on the teaching of literature and other subjects.) How far does the possibility of 'realising' the past (in history) and the distant (in geography) depend upon imagery?"

It should be noticed that the problem of determining image types has been confused with the determination of what might be called 'presentation types.' It has been assumed that so-called 'eye-minded' and 'ear-minded' people are such, because they employ visual and auditory imagery respectively. But *a priori*, there is no reason why there should not be individuals who learn more from visually presented material and who are yet not of the visual image type, and others who learn more from auditorily presented material and who are not of the auditory image type. This paper will be concerned only with image and not with presentation type. In order that the former should be determined, it is necessary to avoid the use of objective tests, which more or less assume the function of imagery. In the present work a purely introspective method was employed, while precautions were taken to ensure the reliability of the introspections used in determining correlation coefficients.

II. EXPERIMENTAL.

The material used in this research falls into the following eight groups:

1. Verbal memory tests.
2. Tests of sensory discrimination.
3. Tests of sensory memory.
4. Tests of general ability.
5. Tests designed to involve the use of imagery.

6. Estimates of visual and auditory ability from introspections.

7. Estimates of scholastic ability according to the results of school examinations.

8. Ranks of children according to scholastic intelligence, practical intelligence, painstaking and social status, drawn up by the teachers of the school.

Most of the experiments were carried out in four classes of the girls' department of an L.C.C. elementary school. One class of boys in the same school was also tested, and as the results did not differ radically from those of the other tests, they were pooled with the girls'[1]. The number of cases varied in the different tests, but in many of them over 150 children were examined. The ages of the children ranged from 7–14 years. All the tests except some of the introspections were carried out in class under ordinary school conditions, all the necessary precautions for the prevention of copying, etc., being taken.

1. *Verbal memory tests.* These were three in number. In each of the three, the children were required to learn one series of 5, four series of 7, and five series of 9 monosyllabic nouns. In the first test the children listened while the experimenter repeated the series three times, and then at a given signal wrote out as many as they could remember, regardless of order. (Auditory verbal memory test.) In the second test the procedure was the same except that the children repeated the words aloud after the experimenter. (Auditory motor verbal memory test.) In the third test the words, printed on cards, were shown one at a time, neither the experimenter nor the class repeating them. (Visual verbal memory test.) The motor factor probably occurs in all three tests, as it is very doubtful if the endeavour to suppress articulation in tests 1 and 3 was successful. The auditory motor verbal memory test was doubly auditory, as the children not only heard the experimenter's voice, but their own voices too.

2. *Tests of sensory discrimination.* There were seven of these, three auditory and four visual.

(i) *Discrimination of pitch.* Two tones were sounded on a monochord one after another, and the children were asked to write whether the second tone was higher or lower than the first. They were quite familiar with the terms 'higher' and 'lower' as applied to the pitch of musical notes, for all the children had been trained from the Infants' School to sing from Tonic Sol-fa modulators on which the higher notes are at the top and the lower notes at the bottom.

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[1] I here express my great indebtedness to Miss H. E. Lacy, headmistress of the Girls' School, who not only allowed me to experiment in her school, but continuously encouraged and helped me. My thanks are also due to Mr D. J. Restall, headmaster of the Boys' School, and to the teachers in whose classes I experimented.

(ii) *Discrimination of rate.* Two rates were given on a metronome one after another, and the children were required to write down if the second was quicker or slower than the first.

(iii) *Discrimination of musical phrases.* Two simple phrases of music were played on a piano one after the other, then one of the two was repeated and the children were required to identify it either as the first or the second.

(iv) *Discrimination of colour.* Two methods were used in this test. The colours used were obtained by mixing two colours not differing greatly in hue, by means of a wheel. In the first method two discs were used, one as a constant and the other as a variable. When in rotation, the discs were uncovered and the children compared them and then wrote down which of the two original colours the variable contained most of as compared with the constant. As the results of this test were unsatisfactory, the method of experimenting was changed. Five discs were used instead of two, one constant and four variables. One of the four variables was the same as the constant and the children were required to write down which they judged it to be. The reliability of the results obtained by this latter method was very much higher than that obtained by the first. The details of these methods are given in an earlier paper[1].

(v) *Discrimination of the size of angles.* A number of solid black angles were pasted on sheets of white cardboard; two of these were shown one after the other, and the children required to write down if the second angle was larger or smaller than the first.

(vi) *Discrimination of patterns.* In this test a series of patterns numbering 1–17 was used. Each pattern was made by a combination of 32 units (black ovals) as shown in the figures. The dotted lines give the basis on which the patterns were constructed; but as they were drawn in pencil, they did not show in the patterns when actually used. Each pattern differed from the preceding one by the displacement of two units. Thus, as will be seen by comparing the figures, pattern 2 is derived from pattern 1 by displacing a unit from the third square from the left in the top row to the first square in the second row, and displacing in the third row a unit from the fourth square to the first, and so on, no place from which a unit had been removed being again occupied. Thus patterns bearing consecutive numbers differed by two units; those whose number differed by two, had 2 × 2 = 4 units displaced and so on. Hence patterns 1 and 17 differed by 2 × 16 = 32 units, *i.e.* all the units were displaced and the two patterns had nothing in common. In the experiment the children were shown two patterns one after another for about five seconds each, then a third one was shown and they were required to identify it as either the first or the second. All the cards were duplicated in order to prevent identification by means of accidental marks. One set was used for learning and the other for identification.

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[1] This *Journal*, 1914, VII. 64.

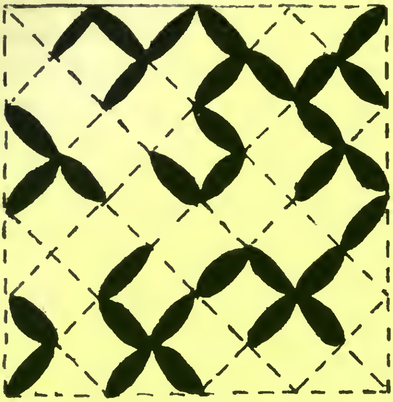

Fig. 1. Pattern 1.

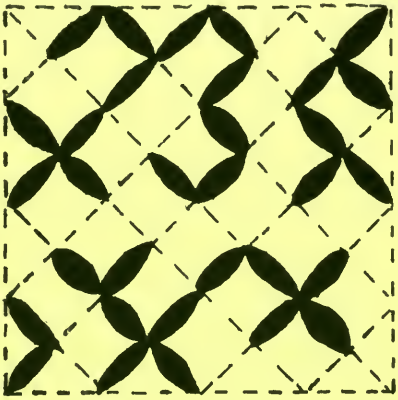

Fig. 2. Pattern 2.

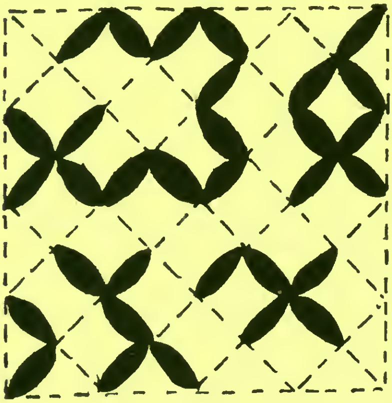

Fig. 3. Pattern 3.

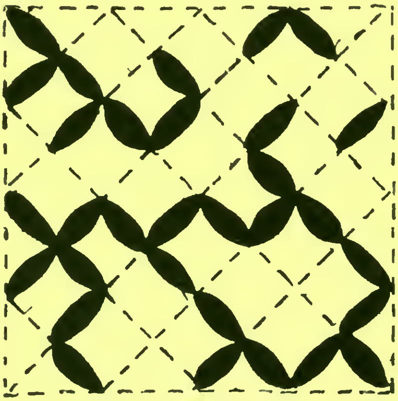

Fig. 4. Pattern 17.

(vii) *Discrimination of groups of colours.* Thirteen groups of colours were used, each consisting of twelve squares of different colours arranged in three horizontal rows of four each. Thirteen colours in all entered into the composition of these groups. The groups, which were numbered 1–13, were constructed on the following plan: After the construction of group 1, group 2 was derived from it by replacing any one colour taken at random by the thirteenth colour, that had been left out in group 1. Group 3 was derived from group 2 in a similar manner by substituting for any one of the 11 unchanged colours, the colour that had been left out of group 2, and so on. In this way any group differed from any other group next in number by one colour only; from those next to these by one colour and one difference of relative position of the colours; and from those next but one to these by one colour and two differences of relative position of the colours, and so on. Thus group 6 differed from groups 5 and 7 by one colour each, from groups 4 and 8 by one colour and one difference of position each, and so on. All the cards were duplicated and the experiment was carried out in the same way as the pattern test.

The usual plan was to experiment with each class of children for half an hour each week. During this time they were given several different tests one after the other. For example, three series of words would be learnt, ten judgments made in each of the first and second discrimination tests, and six judgments in as many as possible of the remaining discrimination tests. Each test was continued until a sufficient degree of reliability had been obtained, or until there appeared no prospect of obtaining good reliability. A series of sufficient length to give a good reliability coefficient consisted on an average of 60 judgments.

3. *Sensory memory tests.* This group comprises seven tests to correspond with those of the previous group. One stimulus in each of the seven different kinds of sense material was given to be remembered, and the children were examined an hour afterwards. Two answers, one in the morning and one in the afternoon, were obtained each day with regard to each kind of sense material, and the experiments were continued for about three weeks with each class of children. Thus, on an average, thirty answers from each child were obtained. In the cases of memory of pitch, rate and musical phrases, three stimuli were given successively in each, and the children were required to identify one of three with the stimulus given an hour previously. In the cases of memory of colour, angles, patterns and colour groups, one stimulus in each was given to be remembered, and then, an hour later, they were given four simultaneously in each, one of which they had to identify as having been given previously.

In the discrimination tests the general plan was to make the successive member of each series of gradually increasing difficulty. The successive series also, from one experimental day to another, were as a whole, of increasing difficulty. In the case of the memory tests, a series requiring 15 judgments of increasing difficulty was given and this was repeated to obtain 30 judgments in all.

4. *Tests of general ability.* The tests selected were chosen with a view to their involving to a large degree the general factor '*g*' common to all mental performances, and to their serving therefore as a measure of it. It was further hoped, also, that their performance would involve no large specific factor common to themselves, or occurring in the performances of any other of the tests used, *i.e.* that they would have no large specific correlation either with each other, or with any other of the abilities tested[1].

The tests were two in number. In the first, a number of words such as wet, smooth, lazy, etc., were read out at a given rate by the experimenter, while the children endeavoured to write down the opposite of each word as it came. In the second test, each child was given a book consisting of five or six pages. On each page, three series of words were written. The children were required to re-arrange these words without additions or omissions to form three sentences. A definite time was allowed for each page, after which the next was attempted.

5. *Tests designed to involve the use of imagery.* An endeavour was made to discover some tests whose performance seemed to depend upon visual imagery. Such tests, it was thought, would give high correlations with visual imagery according to the introspections. Two tests were selected. In the first, twenty drawings consisting of five or six straight lines joined in a variety of irregular ways were made upon separate cards. The cards were each exposed for about one second, and the children were required to draw on some paper before them as much as they could remember of the figure. This test was chosen, because according to the introspections of two psychologists, on whom it was tried beforehand, the clearness and duration of the primary memory image seemed to be important. In the second test a number of pieces of paper were folded into halves, and in each piece a hole was cut in the middle of the folded edge. These were shown to the children for a short time and then they were required to draw the hole as it would appear if the paper were unfolded.

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[1] For further information concerning '*g*' and specific correlations see "General Ability," by B. Hart and C. Spearman, this *Journal*, March, 1912, v. 51.

6. *Introspections concerning visual and auditory imagery.* The introspections were obtained from the children by a series of simple questions. The questions were arranged so that from the answers four sets of marks could be obtained with regard to the clearness of (1) spontaneous visual imagery, (2) spontaneous auditory imagery, (3) voluntary visual imagery, (4) voluntary auditory imagery. In three classes of girls the introspections were obtained individually. With one class of girls answers were obtained first collectively and then later individually. In the case of the boys they were obtained collectively.

The questions used varied slightly from class to class, according to the age of the children, but each series was similar in nature. The following set of questions on visual imagery will serve as an example. The questions on auditory imagery were similar in character.

Spontaneous visual imagery.

1. Repeat these words after me three times, and then write out in any order as many as you can remember: plate, dog, bag, clock, chair.

When you were trying to remember these words, did you picture any of the things they stand for, or did you picture the words as written?

Was what you pictured quite clear, or fairly clear, or only dim?

(These questions were repeated with a list of nine words.)

2. A boy had 35 marbles, he lost 17, how many had he left?

When trying to think of the answer,

did you picture the boy? Clearly, fairly clearly, or dimly?
did you picture the marbles? Clearly, fairly clearly, or dimly?
did you picture the numbers? Clearly, fairly clearly, or dimly?

(These questions were repeated in relation to another problem.)

3. Repeat the following little story after me twice, and then tell me as much as you can remember of it. "Outside the church door stood an old beggar, leaning on his crutch. John gave him one of his silver shillings and then went on his way."

When you were trying to remember this,

did you picture the church door? Clearly, fairly clearly, or dimly?
did you picture the beggar? Clearly, fairly clearly, or dimly?
did you picture John? Clearly, fairly clearly, or dimly?

Voluntary visual imagery.

1. Think of your breakfast table.

Can you picture it? Clearly, fairly clearly, or dimly?
Can you picture the cups? Clearly, fairly clearly, or dimly?
Can you picture the tea being poured out? Clearly, fairly clearly, or dimly?

2. Think of the fire engine.

Can you picture the firemen? Clearly, fairly clearly, or dimly?
Can you count them? Clearly, fairly clearly, or dimly?
Can you picture the horses galloping to a fire? Clearly, fairly clearly, or dimly?

3. Think of the story of Cinderella.

Can you picture Cinderella in rags before the kitchen fire? Clearly, fairly clearly, or dimly?
Can you picture her at the ball? Clearly, fairly clearly, or dimly?
Can you picture her dressed like a princess? Clearly, fairly clearly, or dimly?

4. Think of the wall of the class-room.

Can you picture its colour? Clearly, fairly clearly, or dimly?

(These questions were repeated with reference to a fire engine, a blue sky, an orange, a school history book, and the Union Jack.)

5. (Six cards, on which were fixed circles of various colour, were shown to the children, and they were asked to describe the clearness of the visual image when these were covered.)

Before putting these questions to the children measures were taken to ensure that they thoroughly understood what was required of them. No case occurred of a child seeming to experience any difficulty in understanding what was meant by 'picturing.' This seems to indicate that no case was discovered of a child without any visual imagery at all, although several were found in whom it was very scanty. It is very possible, however, that many children overrated the clearness of their imagery; hence, before the experiment, each child was asked to look at a particular object in the room and then to shut her eyes and compare the clearness of the image with that of the object, and an endeavour was made to make the meaning of the terms 'clear,' 'fairly clear,' and 'dim,' as definite as possible. 'Clear' was to mean that the image was as plain as the object itself. 'Fairly clear' was to mean that the image was not so plain as the object, but that it could be maintained without much effort. 'Dim' was to mean that the object could be pictured, but that it was 'shadowy' and kept coming and going. The children were repeatedly reminded of these meanings during the course of the experiments. The questions were more often than not supplemented by phrases calculated to keep the required meaning before the mind of the child; for example, "Can you picture it, or are you only just thinking about it?"

To be sure that the children understood what is meant by an auditory image was not nearly such a simple matter. The fact that there is no such simple word as 'picturing' to describe the auditory process, somewhat adds to the difficulty. Some such clumsy phrase as 'hearing in the head' has to serve. Some children too, especially those who go to Sunday school, seem to identify an auditory image with conscience. But in every case a little extra explanation seemed to suffice to make the matter clear. The usual method of explanation was as follows: A book was shown to the child, and she was asked if she could picture it, not merely think about it, but really see it in her mind or brain. Then the book was dropped upon the floor, and the child was asked if she could not only think about the sound of the book falling, but really hear the sound in her head; as she had seen the book in her head. As a rule, this was quite sufficient to call forth a beaming smile, indicating understanding, and to enable the experimenter to continue the introspections without further difficulty.

The clearness of each child's imagery was estimated by giving three marks for each clear image, two for each fairly clear, and one for each described as dim. In this way marks were obtained for imagery which could be correlated with those obtained in other performances.

7. *School examination marks.* In most cases, these marks were those obtained at the term examinations, but where possible they were supplemented by marks given by the class teachers in weekly tests. The subjects included were: reading, spelling, composition, arithmetic, science, history, geography, needlework, writing and drawing.

8. *Teachers' lists.* For each class of children examined, two teachers were asked to draw up lists in order of scholastic intelligence, practical intelligence, painstaking and social status. The two particular aspects of intelligence were specified, as several teachers experienced difficulty in placing certain children in a more general list. For example, children who show little ability in their school-work are

described as being very capable in managing things relating to the class-room.

The lists drawn up by the two teachers were correlated with one another and the figure obtained was treated as the reliability coefficient[1]. The correlations between the two estimates for each of the four qualities were: scholastic intelligence ·79, practical intelligence ·60, painstaking ·62, social status ·72. Both compilers being largely influenced by the same term examination lists, probably accounts for the correlation between the two estimates of scholastic intelligence being the greatest. Attention has been drawn by other writers to the fact that the correlation between two such lists is partly spurious, owing to their not being based on completely independent data.

III. RELIABILITY.

Procedure. Each experiment was repeated several times. The marks obtained were divided into two groups and added up so that two sets of measurements were obtained for each ability tested. The groups were arranged so that any regular errors such as those due to practice and fatigue would be eliminated in the process of correlation. Thus, in a case when an ability had been measured eight times, two groups of marks were obtained by adding the results of measurements 1, 3, 5 and 7, and 2, 4, 6 and 8. In some cases two groups were formed by taking 1, 2, 7 and 8, and 3, 4, 5 and 6. The correlation between the different performances was obtained by means of the product-moment formula, which was then corrected in the usual way to eliminate errors of observation[2].

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[1] See section on Reliability.

[2] "Correlation Calculated from Faulty Data," by C. Spearman, this *Journal*, III. Pt. 3, 1910, p. 276.

For p.e. p. 294.

In order to shorten the work, both the correlation and the correction can be done in one step by using the following modified formula:

$$r_{xy}=\frac{\Sigma x^2{}_{a+b}+\Sigma y^2{}_{a+b}-\Sigma(x_{a+b}-y_{a+b})^2}{4\sqrt{[\Sigma x_a{}^2+\Sigma x_b{}^2-\Sigma(x_a-x_b)^2][\Sigma y_a{}^2+\Sigma y_b{}^2-\Sigma(y_a-y_b)^2]}},$$

where r_{xy} = required correlation.

$\Sigma x^2{}_{a+b}$ = sum of the squares of the sums of the deviations from the average in the two measurements x_a and x_b.

$\Sigma y^2{}_{a+b}$ = same for y.

$\Sigma(x_{a+b}-y_{a+b})$ = sum of the squares of the difference between the sums of the deviations of the two measurements of x, and the sums of the deviations of the two measurements of y.

The reliability coefficient was obtained by correlating two measurements of the same ability[1].

Small individual variations in the ability tested. It soon became obvious that reliability is much more easily obtained in some tests than in others. It was easy to obtain reliable measurements in the verbal memory tests and in ability to discriminate the pitch of notes, but in others, such as discrimination of rates or angles, a satisfactory reliability was rarely attained. This lowness could be attributed to several causes. One of these was the small individual variations in the ability tested. An example of lowness of reliability due to this is that of discrimination of rate. This test was carried out by the same method as that of discrimination of pitch, but the reliability in the case of pitch was not only much higher, but much more quickly obtained. At first this was thought to be due to the rate test being too easy; therefore it was repeated and made more difficult. The result was that the number of correct answers became considerably less, but nevertheless, the individual variations still remained very small. Further increase of difficulty gave large random variations from one test to another, showing that the degree of difficulty was too great. Discrimination of angles is another example of a test from which, for the same reason, it is difficult to obtain any reliable results. Although the test was carried out a great number of times and in two ways, no large individual differences were obtained.

Defects of method. Another cause of lowness of reliability is defective method of experiment. The earlier tests of ability in discriminating colour afford an example of this. The average reliability obtained from the five classes with this method was, in spite of many repetitions, only ·52 in the case of yellow, and ·37 in the case of blue. But as soon as the second method was introduced, reliabilities ranging from ·75 to ·89 were obtained with considerably shorter series[2].

Influence of attention. A third cause of low reliability is variation in attention due to the nature of the material used in the test. The

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[1] This could be obtained approximately by means of the following modified formula:

$$r_{x_a x_b} = \frac{\Sigma x_a^2 + \Sigma x_b^2 - \Sigma \overline{x_a - x_b}^2}{\Sigma x_a^2 + \Sigma x_b^2}.$$

The approximation obviously consists in replacing in the denominator $2\sqrt{\Sigma x_a^2 \Sigma x_b^2}$ by $\Sigma x_a^2 + \Sigma x_b^2$, which is sufficiently accurate as long as Σx_a^2 has nearly the same value as Σx_b^2.

[2] For further information with regard to this method see this *Journal*, "An improved Colour-wheel," May, 1914, 64.

discrimination of patterns and colour group tests are examples of this. Both these tests were repeated by the second method used in the colour test; that is, the children were shown one pattern or colour group and then were required to identify it as one of four patterns or colour groups. But in neither case was there any great improvement in reliability. In both methods the children were warned that unless they attended to the whole of the pattern or colour group and not merely to one part, they could easily make errors. Nevertheless, it is highly probable that attention was not always given to the whole and that therefore a large random error was introduced. Tests involving this kind of material, when applied to children, would probably never give a very satisfactory reliability. In the sensory memory tests also it was difficult to obtain any high reliability, and this was probably due to the flagging of attention towards the end of the series. All these memory tests were continued during a period varying from a fortnight to three weeks. After the novelty of the tests had worn off, it is highly probable that certain children did not always give the full amount of attention to the tone, rate, colour, etc., to be remembered.

Mass v. individual experiments. Some writers have raised objections to mass experiments on the ground that less reliable results are obtained. Of course, there are certain tests which cannot be so readily applied *en masse* as individually. But where the method does allow of ready application, mass experiments have the advantage of having been carried out under more natural conditions. Children who are inclined to be nervous are much more disturbed by individual than by mass tests.

The auditory motor verbal memory test and another involving the association of numbers with words were carried out both in class and individually for the purpose of comparing the two modes. The reliabilities of the class tests were ·78 and ·67, while those of the individual tests were ·69 and ·57. In both cases the reliability of the individual test was lower than that of the class test. The experience of Mr Burt in this matter is similar, for he writes: "The reliability of the group test carried out was ·90, unexpectedly high....The individual test, strange to say, gave less satisfactory results. Extending as it did over the greater portion of the school term, the application of this test individually doubtless caught the children under varying conditions of health and fatigue; and to these variations the unsatisfactoriness seems mainly due[1]."

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[1] "Experimental Tests of the Mental Processes and their Relation to General Intelligence," *J. of Exp. Ped.*, Nov. 1911, 103.

Another factor also influencing such tests is nervousness. In the individual tests, signs of nervousness such as flushing frequently appeared, due often to an over-anxiety to do well, especially when under the immediate eye of the experimenter. In one extreme case, a child burst into tears because she had not remembered as many words as she had wished, although she had been told at the beginning (the experimenter being aware of her disposition) that it did not matter if she forgot the words as long as she did her best. Nervousness of this kind does not occur in mass experiments, as the subjects do not feel that they are the sole object of the experimenter's attention.

IV. Examination of the Validity of the Children's Introspection.

Criticism. Many psychologists have maintained that it is impossible to obtain reliable introspections, even of a simple character, from children. For instance, Colvin and Myers consider that children are incapable of making introspections with any degree of accuracy, and base their objection on the following: A test involving certain distractions was given to a hundred school children in the 4th and 5th grades. Together with the tests was given a list of questions inquiring concerning the effect of the distraction upon their power to memorise. Of the answers given a little over 50 % agreed with the actual results obtained from the tests. From this figure they conclude that children are incapable of any accurate introspection. Nevertheless, in spite of their strong avowal of the inability of children to introspect they actually resort to it and draw conclusions from introspections. For example, they write: "In these grades too (higher grades) an attempt was frequently made to assign names to meaningless characters, a procedure reported less often in the lower grades[1]." From these introspections it was inferred that there is a general tendency towards the growth of a verbal type with increase in age.

Criteria. Prof. Külpe has suggested that in order to determine whether an introspection is valid or not, it should be compared with already established facts, with introspections of other subjects, with other introspections of the same subject, and, also, if the introspection is concerned with an external stimulus, with the stimulus itself. An endeavour should also be made to determine the subject's ability to express himself concerning his experiences[1]. In order to establish the validity of the children's introspection, an attempt has been made to follow these suggestions.

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[1] *Psychol. Rev., Monogr. Suppl.* II. 1909, "Development of Imagination in School Children."

Distraction experiments of Colvin and Myers. One way of comparing the introspections with already established objective facts is the method of Colvin and Myers described above. But before one can tell exactly what their percentage means, it is necessary to know the degree of correspondence to be expected from chance, and this will depend upon the particular method of experiment and the number of distractions given. Without these details the figure 50 % is meaningless.

In order to check the experiments of Colvin and Myers, a series of four tests was carried out with twenty children whose ages varied between 11 and 14, and twenty adults. The children were selected some from the top of the term examination list, some from the middle, and some from the bottom, in order that they should be representative of a whole class. The tests used were the following:

1. Tapping with the left hand, adding in threes and writing down the results with the right hand.
2. Tapping with the left hand, and copying words with the right hand.
3. Tapping with the left hand, and spelling the word 'probability' aloud.
4. Tapping with the left hand and adding up in twos aloud.

In each of these four tests the subjects were instructed to perform both operations as quickly as possible. A minute was given for each test. At the end of the fourth, the subject was to say which performance was felt to be the most distracting to the tapping, then which the least, and finally, to place the remaining two tests in order of distractibility. The order thus obtained was then compared and correlated with the order of the tests according to the number of taps actually made. The results of the test are shown in Fig. 5. The abscissae show the error in the order according to introspection as compared with the actual order. The ordinates show the number of individuals making these errors. Diagram A is for adults and B for the children. The adults have done better than the children, their average correlation between the order according to introspection and the actual order according to the number of taps, being ·74 ± ·068, whilst that for the children is ·48 ± ·115. If, however, the average deviation from the average number of taps is calculated, that for the adults is 30, while for the children it is 20. As the differences in the number of taps for the four tests were greater for the adults, the judgments they made would be correspondingly easier, and a higher correlation for the adults is to be expected. Moreover, the adults tested were mostly students and teachers and belonged to a higher and probably innately more competent class.

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[1] Given by E. Westphal, "Über Haupt- und Nebenaufgaben bei Reaktionsversuchen," *Arch. f. d. ges. Psychol.* xxi. 239.

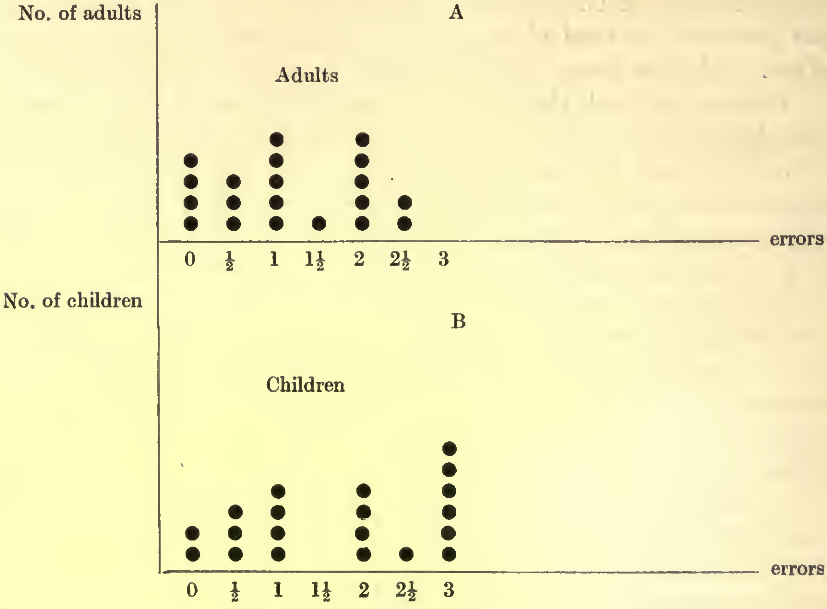

Fig. 5.

If the distributions in the graphs A and B are studied, it will be seen that they are similar in character except for the extreme right ends. Forty-five per cent. of the children are above the average for the adults, and thirty-five per cent. of the adults are below the average for the children. On the whole, the differences between the two sets of results do not seem excessive, and are certainly insufficient to warrant the total rejection of the introspection of the children.

Agreement between different tests of imagery type with children and adults. Three more tests were carried out with the same children and adults, and the results compared. They were the following:

1. Kraepelin's test. The subject was required to write out a list of objects characterised (*a*) by their colour, (*b*) by their sound. Time: three minutes each list.

2. Secor's test. A series of 20 words was written on slips of paper. The subject looked at each word in turn and reported the imagery in each case. The test was repeated with a second series of 20 words read instead of being shown. Only visual and auditory images were taken into consideration, one mark being given for each image reported.

This test is a slightly modified form of Secor's original test. The words chosen were the names of objects which could be represented either by an auditory or a visual image. The subjects were not required to make any associations, but merely to report their spontaneous images. The modification was made in order that the test might be a purely subjective one as compared with the other two, which are objective.

3. Sensory similarity in recall. (*a*) The subject was given the word 'tone' and asked to write a list of words which rhyme with it; (*b*) the subject was asked to write a list of words ending in 'ine,' regardless of pronunciation. Time: three minutes each list.

In Kraepelin's test it is assumed that a visualiser will find it easier to write a list of objects characterised by their colour than by their sound, while the reverse will hold of a subject of the auditory type. In Secor's test it is assumed that a visualiser will report more visual and a subject of the auditory type more auditory images. These two tests have been used with four observers, and have been described as giving satisfactory results[1]. In the third test it is assumed that subjects of the auditory type will find the first list easier, while visualisers will find the second easier. Both Secor's and test 3 are recommended by Angell as suitable for determining image types[2].

If all these assumptions are true, then the inter-correlations between all the tests determining the visual type, namely tests 1*a*, 2*a* (visual images), 3*b*, and the inter-correlations between tests determining the auditory type, namely 1*b*, 2*b* (auditory images) and 3*a*, should be high, while the correlations between tests determining different types should be low.

Tables I and II show the correlations of tests 1, 2 and 3 for adults and children respectively. From each test two measurements *a* and *b* for each individual were obtained. Thus altogether there were six series of measurements to be correlated. The italicised correlations are those which, if the tests really determine types, should be high. In both

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[1] Bell and Muckenhoupt, "Comparison of Methods for the Determination of Ideational Types," *Amer. J. of Psychol.*, 1906.

[2] "Mental Image," *Psychol. Rev., Monogr. Suppl.* XIII.

TABLE I. *Adults.*

The figures in heavy type under the correlations are the probable errors.

		1*a*	1*b*	2*a*	2*b*	3*a*	3*b*
		Kr. Red	Kr. Tone	Secor's Vis.	Secor's Aud.	'tone'	'ine'
1*a*	Kr. Red		·53 **·103**	*– ·19* **·149**	– ·09 **·15**	·38 **·129**	*·22* **·144**
1*b*	Kr. Tone	·53		– ·08 **·15**	*– ·09* **·15**	*·10* **·15**	·22 **·144**
2*a*	Secor's Vis.	– ·19	– ·08		·40 **·127**	·29 **·138**	*·32* **·137**
2*b*	Secor's Aud.	– ·09	·09	·40		*·02* **·151**	·03 **·151**
3*a*	'tone'	·38	·10	·29	·02		·82 **·049**
3*b*	'ine'	·22	·22	·32	·03	·82	

TABLE II. *Children.*

		1*a*	1*b*	2*a*	2*b*	3*a*	3*b*
		Kr. Col.	Kr. Sound	Secor's Vis.	Secor's Aud.	'tone'	'ine'
1*a*	Kr. Col.		·52 **·133**	*·00* **·15**	– ·11 **·149**	·21 **·145**	*·39* **·128**
1*b*	Kr. Sound	·52 **·113**		– ·21 **·145**	*– ·26* **·142**	*·27* **·141**	·34 **·134**
2*a*	Secor's Vis.	·00 **·15**	– ·21 **·145**		– ·15 **·146**	– ·41 **·127**	*– ·24* **·142**
2*b*	Secor's Aud.	– ·11 **·149**	– ·26 **·142**	– ·15 **·146**		*– ·09* **·149**	– ·21 **·145**
3*a*	'tone'	·21 **·145**	·27 **·141**	– ·41 **·137**	– ·09 **·149**		·33 **·134**
3*b*	'ine'	·39 **·128**	·34 **·134**	– ·24 **·142**	– ·21 **·145**	·33 **·134**	

tables, however, they are low; the average of the adults is ·09; for the children ·03. All the differences between corresponding italicised correlations in the two tables are within the limit of three times the probable error. From these figures it appears that these tests are not dependent for their success upon visual and auditory imagery as assumed. The spontaneous introspections of the adults seem to support these figures. Most of these subjects at the end of the tests desired to know their object. On being told that one aim was to determine the subject's particular image type, more than one considered the tests unsuitable for this purpose, saying that in tests 3*a* and 3*b*, Table I, at least, similar imagery was present in both cases. Test 3*b* can be carried out by rhyming just as test 3*a* can. One pronunciation of the syllable 'ine' can be taken and a list obtained by rhyming. Then another pronunciation can be taken and another list obtained by rhyming. The high correlation between tests 3*a* and 3*b* in the case of the adults confirms this. In the case of the children it is much lower, but still higher than all but one of the italicised correlations. Again the correlations between tests 1*a* and 1*b*, which according to assumption should be low, are comparatively high, namely ·53 and ·52 in the adults' and children's tables respectively. With regard to the children's ability to introspect as compared with the adults, the tables are in quite satisfactory agreement. The occurrence of low correlations where there should be high ones, in both, is due to the tests being useless for the purpose for which they are intended.

Correlations between two sets of introspections of the same children. In order to compare two sets of introspections by the same subjects, in the case of one class of children, introspections were obtained both in class and individually. They were obtained first collectively, each child writing down the answers to the questions. Six months later, the same children were examined individually, the same questions being used. On the second occasion not one child could remember any of the questions that had been asked on the first, and some had even forgotten the event altogether. The following correlations were obtained between the two sets of results:

Voluntary visual (class) and voluntary visual (individual) = ·63.
Voluntary auditory (class) and voluntary auditory (individual) = ·60.
Spontaneous visual (class) and spontaneous visual (individual) = ·44.
Spontaneous auditory (class) and spontaneous auditory (individual) = ·75.

Let us compare these figures with analogous ones that have been obtained under extremely favourable circumstances. We may take the estimates of intelligence by teachers of the children they are continually observing. The correlation between two estimates of the intelligence of the same children by the same teacher, the second being made nine months after the first, was ·66, a figure of the same order as those obtained above for the children's introspections of imagery. Although these results cannot prove that children do not make regular errors in their introspections, they at least prove that their answers are not random ones.

Comparison of children's introspections with those of trained psychologists. In order to further test the ability of the children to introspect, three experiments were made upon seven children and three trained psychologists, and the introspections of the two groups were compared. The tests used were:

1. The subject was shown for five seconds a drawing consisting of a number of lines, and then required to reproduce it.

2. The subject was shown for two seconds a piece of paper folded in halves, in the folded edge of which a hole was cut, and then required to draw the hole as it would appear if the paper were unfolded.

3. The subject was shown one after another seven or nine words printed on cards. The series was shown three times and then the subject was required to reproduce it. Introspections were made after each test. The children were tested first. Their introspections were given verbally and taken down by the experimenter. Each child was first of all asked to make statements as to how she remembered the drawing or words; and then, if necessary, if she had had any pictures in her mind, and if so, if they had been useful or not. The psychologists merely wrote down their introspections after each test.

As was to be expected, the introspections of the children were exceedingly simple, as compared with those of the psychologists. The main purpose of the comparison was, however, to see if any points given by the psychologists were also given spontaneously by the children. The following are fair samples from the introspections of both, showing such points of correspondence.

Psychologists.	*Children.*
Test 1.	
A. W. As soon as the picture was shown, I felt it was exceedingly easy and that I could reproduce it without difficulty. When it was covered over, there remained a distinct primary memory image, visual in the full sense that I could actually see the lines and followed them as I was drawing. I began left hand top, but when I got to the point indicated by x in the figure, I was lost, for the image had gone. I endeavoured to ecphore the remainder of the figure and then an image came up again. I still call this visual, but it had nothing of the sense perception of the primary memory image. I think in my case these images were of first-rate importance for the reproducing of the figure.	*D. I.* (from notes). Pictured it—then picture went—then drew lines where she thought they should be. Picture was helpful.
J. C. F. Had very distinct primary memory image, but this faded very rapidly. Continued to have fairly distinct image of upper part of figure, but imagery seemed to be of little help with the lower.	*M. H.* Pictured it, but saw part only as clear as original. Picturing helped in drawing.
Test 2.	
A. W. When it (folded paper with hole) was shown, it appeared an easy matter to draw the hole. I found, however, that some kind of reasoning was present. I said to myself—the other half is the mirror image of the half shown. When drawing there was faint visual imagery, but it did not seem to be of such importance as in the first experiment.	*D. L.* (tested three times). Pictured one out of three—did not think picture helpful. Thought of one half and put on the other half. *E. W.* No picture—put down one half and worked out other half from that.
W. L. Thought I could do this, as I knew that the other side must correspond with the one seen. Visual image of the cut, construction accordingly of the rest.	*D. T.* No picture—thought about it—drew what she saw and then added other half upside down. *A. N.* Pictured it—drew top part and then put on the other part. Picture helpful.
Test 3.	
W. L. Pleased on finding there was kind of rhyme. Words recalled kinaesthetically.	*M. H.* Remembered chiefly by rhythm. Pictured some, but wasn't helpful. Felt as "if she were saying all the words to herself."
J. C. F. Had auditory and motor and in some cases visual images, but they did not seem to play quite such an important part in recollection as they did in the previous series. Recollected partly by the meaning of the words and (at least in one case) by artificial associations.	*D. L.* Visual images—but doesn't think they were helpful. Motor and auditory images — were helpful. *A. N.* No pictures—just thought about them.

Considering their lack of training and limited powers of expression, the above comparison is a satisfactory one from the point of view of the children. In no case did any real discrepancies occur.

After all, simple introspections of the kind made use of in this work are really easy. Satisfactory answers with regard to imagery have even been obtained by the questionnaire method. Galton says with reference to the answers he obtained: "The conformity of replies from so many different sources...has convinced me that it is much easier than I had anticipated to obtain trustworthy replies to psychological questions. Many persons, especially women and intelligent children, take pleasure in introspection and strive their best to explain their mental processes." In the present case all the children had, at some time or other, been the pupils of the experimenter, who could therefore read their facial expressions and determine quite easily the degree to which they understood, and the value of the answers which they gave. Many of them, when once they had understood what was required of them, supplied a good deal of information quite spontaneously, and the amount of pleasure it appeared to afford them was quite amusing. Not a single case occurred of a child who appeared to have failed to understand or to be unable to answer. Of course, some children required more explanation than others, and doubtful cases were settled by cross-questioning.

As the value of the results to be considered in the next section is dependent upon the validity of the introspections of the children, and several objections have been raised against the use of such introspections, it has been very necessary to carry out the various tests which have been described in this section, and to consider the results in a comparatively lengthy manner. As the results of all the comparisons which have been made are quite satisfactory, we can now pass on to consider the correlations of imagery with the other tests described in Section II.

V. RESULTS.

The correlations of imagery with the different groups of tests are given in Tables III, IV, V, VI, VII, VIII and IX, which will each be considered.

1. *The correlations between the different classes of imagery* (Table III). As Betts also found, these are higher than the correlations of imagery with any other ability. The correlation between voluntary visual and voluntary auditory imagery is the highest and is ·60, that of Betts being ·53. However, the conclusion of Betts that the wide and rather even distribution of the power of imagery among the different types seems to leave no foundation for such classification as 'eye-minded,'

'ear-minded,' 'muscle-minded,' etc., is rather premature, since it is based on the correlations between different types of voluntary imagery, and not on those between different types of spontaneous imagery. Betts does not give the latter correlations; but in these experiments the correlation between spontaneous visual and spontaneous auditory imagery was ·32, which is considerably lower than that between the voluntary types. As the difference between the voluntary and spontaneous types of imagery is that indicated by the adjectives used to distinguish them, the decrease in the correlation between the spontaneous as compared with the voluntary types means that there are individuals who have a tendency to employ one type of spontaneous imagery more than the other. The correlation is, however, still distinctly positive and does not justify the assumption of any very definite image types. The difference between spontaneous and voluntary imagery is, however, a matter which requires further investigation.

TABLE III.

	Vol. Vis.	Vol. Aud.	Sp. Vis.	Sp. Aud.
Vol. Vis.		·60 ·038	·22 ·057	·38 ·052
Vol. Aud.	·60		·29 ·055	·48 ·046
Sp. Vis.	·22	·29		·32 ·055
Sp. Aud.	·38	·48	·32	

2. *Correlations of imagery with sensory discrimination and sensory memory.* These correlations, which are given in Tables IV and V, are all low but mostly positive. The average of Table IV is ·094 ± ·015, and that of Table V ·091 ± ·021. In both cases the correlations, although low, are significant, as they are well over three times the probable error. From these averages it would appear that imagery is slightly helpful in the processes of discrimination and memory. There is, however, an inverse possibility and that is, that those who remember better have a tendency to produce better images on that account. (It should be noted that the only difference between discrimination and memory may be that the time interval between the experience and its reproduction in the one case is exceedingly short, while in the other it is much longer.)

TABLE IV.

	Vol. Vis.	Vol. Aud.	Sp. Vis.	Sp. Aud.
Pitch D	·04 ·062	·00 ·063	·10 ·06	−·18 ·06
Rate D	·07 ·061	·14 ·06	·16 ·06	−·05 ·061
Mus. D	·16 ·07	−·07 ·072	·24 ·069	−·13 ·071
Colour D	·01 ·06	·21 ·067	·27 ·06	·16 ·06
Angles D	·01 ·062	·15 ·06	·09 ·061	·25 ·058
Pat. D	·17 ·072	·18 ·072	·08 ·073	−·05 ·073
Col. Groups D	·08 ·072	·10 ·072	·25 ·068	·20 ·071

Av. = ·094 ± ·015

TABLE V.

	Vol. Vis.	Vol. Aud.	Sp. Vis.	Sp. Aud.
Pitch M	−·05 ·075	−·01 ·075	−·04 ·075	−·07 ·075
Rate M	·16 ·074	·24 ·071	·09 ·075	·01 ·075
Mus. M	·23 ·093	·08 ·095	·33 ·084	·11 ·094
Colour M	·12 ·105	·00 ·104	·10 ·102	−·06 ·103
Angles M	·16 ·073	·26 ·070	·07 ·075	−·02 ·075
Patterns M	·08 ·093	·07 ·095	·06 ·095	·06 ·095
Col. Groups M	·16 ·074	·17 ·074	·11 ·074	·12 ·074

Av. = ·091 ± ·021

In Table IX are given the correlations calculated after pooling the marks obtained in certain tests in each group. Thus voluntary and spontaneous visual imagery were pooled and similarly voluntary and spontaneous auditory imagery. The marks for verbal memory were obtained by pooling the three verbal memory tests; auditory discrimination by pooling those for pitch, rate and musical discrimination; for visual discrimination by pooling those for colour, angles, patterns and colour groups; for visual and auditory memory by pooling those for the corresponding memory tests. School marks were obtained by pooling those for the separate subjects. In all these cases the actual marks were pooled and not the average correlations taken, in order that the observational error might be reduced.

It will be seen from the correlation of visual and auditory imagery with visual and auditory discrimination and memory (Table IX) that only in the case of the visual performances does there seem to be any tendency to higher correlation with the corresponding imagery. Visual discrimination correlates more highly with both kinds of imagery. The four correlations of the memory tests with imagery are more equal in value. Taking into consideration all these figures, it appears that imagery may function slightly in visual discrimination, and visual and auditory memory, but not in auditory discrimination. On the whole, visual imagery is more important than auditory.

As from the nature of the tests there can be no question of the vicarious functioning of imagery here, any significant correlation of a visual test with auditory imagery, or *vice versa*, must be due to the correlation between the two types of imagery.

One of the criticisms upon Betts' work has been that he has ignored verbal imagery and that this is the cause of the low correlations of imagery with every mental performance. This objection cannot be raised here, concerning either Table IV or V, for in the tests from which these correlations were obtained, there was no possibility of descriptive verbal images occurring, for the differences between the pitches, rates, colours, angles, etc., were too small to allow of it. Another objection, however, might be raised, and that is that kinaesthetic images have not been considered, and that these might have functioned here. It is possible, though not probable, that kinaesthetic images may have come into play in discrimination and memory of pitch and rate, but in the case of discrimination and memory of colour it is difficult to see how such or any other kind of images not already considered could have functioned.

3. *Imagery and verbal memory.* It might be supposed that superiority in visual imagery would correspond with superiority in remembering visually presented material, comparison being made both with the amount remembered by other individuals of inferior visual imagery and the amount remembered by the same individual with a different mode of presentation. If this supposition were true, one would expect that clearness of visual imagery would have a high correlation with memory of visually presented material and a lower one with auditorily presented material; but this is not borne out by the correlations of Table VI. In this table, the correlations of visual imagery with visual verbal memory and auditory verbal memory are both very low, being ·18 and ·15 respectively, the difference between the two being less than the probable error of either. Similar results were obtained with auditory imagery and visual and auditory verbal memory. The average correlation for the group is, however, ·03 ± ·023, a low but significant value.

TABLE VI.

	Vol. Vis.	Vol. Aud.	Sp. Vis.	Sp. Aud.
Aud. Motor Verbal M	·03 ·063	·12 ·062	·24 ·059	·18 ·061
Visual Verbal M	·02 ·062	·00 ·062	·08 ·061	·19 ·06
Aud. Verbal M	− ·07 ·062	·09 ·062	− ·04 ·062	·12 ·062

Av. = ·08 ± ·023

4. *Imagery and the tests designed to correlate with it.* The correlation between these two tests themselves was ·51, and after eliminating the general factor, the specific correlation was ·47. This specific correlation might be expected to be due to both tests being dependent upon imagery. The correlations with imagery are given at the end of Table IX. It will be seen, however, that they are very low. The common factor, then, causing the specific correlation between the two tests is evidently not imagery. It has been found[1] that the technical subjects of the school curriculum show a specific correlation due to a factor which is probably the motor factor. Drawing is one of these technical subjects, and it is more than likely that the drawing element

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[1] These results will be published later.

common to both these tests, and which introduces the motor factor, is the cause of the specific correlation between them. The low correlation of these tests with imagery corroborates the results of some previous and very similar experiments. For example, in his book *Body and Mind*, p. 338, Mr McDougall describes an experiment in which "a number, say ten points of light are thrown simultaneously for a small fraction of a second on a screen. The subject is required to draw a map of the spots." This experiment is very similar to the drawing test which is now being considered. In a footnote Mr McDougall remarks: "There are considerable differences between individuals in respect to the ease with which they achieve such a task, but those who are good visualisers do not seem to excel others."

Another experiment in which similar material was used is one described by Mr Winch in a paper on the "Function of Images[1]." The tests were made upon a class of boys. The material used was of the same nature as that used in the drawing test, but the method was quite different. The boys were divided into two groups, A and B. Designs were exposed for a minute or so and they were required to look at them and then to draw them from memory. A month later fresh designs were shown. On this occasion group B reproduced them after giving a minute or so to the formation of a visual image, whilst group A formed no such image but reproduced at once. The results showed that group B, instead of being better, were slightly worse. The test was repeated in the same way, without warning, a week later, and both groups made the same number of errors, showing that imagery had not been helpful in prolonging memory of form. Thus a similar result has been arrived at by a totally different method.

5. *Imagery and the school subjects* (Table VII). Here again all the correlations are low, and a considerable number are even slightly negative. Science and history have been omitted because the number of cases was much smaller (24) than in the other subjects. The results, however, such as they were, were negative.

Of course, this result does not mean that a lesson with a number of concrete illustrations such as diagrams, pictures, models, etc., is not likely to be much more successful than a mere 'lecture.' But the reason is not that clearer images are developed. One of the chief functions of a teacher is to develop associations between words and their meanings in the mind of the child, between phrases and the relations which they express; and these need not necessarily involve images. Such associations are only built up in the first place by perception, that is, by associating the word or phrase with the actual object or relation. They are slow in their formation and much repetition is necessary to establish them firmly. It is because of these imperfect associations and the consequent imperfect apprehension that the 'wordy' lesson is more or less unsuccessful. Even perception or the memory of perception is only the beginning and not the whole of scientific knowledge. If a child has clear images but incorrect ones, he may fail to understand a lesson, but it is also possible that he may have very clear and correct images at the end of a lesson (for example, a lesson on oxygen during which phosphorus, sulphur, etc., have been burnt), and yet remain almost as ignorant as before, because he has failed to grasp the underlying relations and meanings.

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[1] *J. of Philos., Psychol., and Sci. Meth.*, 1908.

TABLE VII.

	Vol. Vis.	Vol. Aud.	Sp. Vis.	Sp. Aud.
Reading	·00 ·074	·01 ·074	·06 ·074	–·03 ·074
Writing	·06 ·077	·00 ·077	·15 ·074	–·15 ·075
Arithmetic	–·10 ·075	·15 ·074	–·16 ·074	–·09 ·075
Spelling	·02 ·075	·03 ·075	–·01 ·075	–·10 ·075
Composition	–·10 ·074	·16 ·074	–·12 ·074	–·04 ·075
Needlework	·10 ·075	·02 ·075	·04 ·075	·05 ·075
Painting	·20 ·073	·21 ·073	·17 ·073	·05 ·075
Geography	·02 ·094	·30 ·086	–·25 ·089	·05 ·094

Av. = ·022 ± ·069

Of the four correlations of arithmetic with imagery, three have low negative values. This is in agreement with the results of Thorndike and Betts (p. 458). The introspections of Starch's subjects (p. 456) to the effect that their improvement in mental arithmetical tests was partly due to the acquisition of the ability to visualise the arithmetical operations cannot be relied on, as the different subjects were not compared with regard to their visualising powers and their performances in arithmetical tests.

6. *Imagery and the teachers' estimates* (Table VIII). More than half of these correlations are slightly negative. With regard to scholastic ability, Betts obtained a similar result, for he says: "There seems to be an almost entire absence of correlation between ability in imagery of the voluntary sort, and ability in college studies as measured by teachers' markings. Whatever little correlation there is, is almost entirely negative."

TABLE VIII.

	Vol. Vis.	Vol. Aud.	Sp. Vis.	Sp. Aud.
Scholastic Intelligence	-·09 ·06	-·04 ·06	·08 ·06	-·11 ·06
Practical Intelligence	-·03 ·06	-·06 ·06	·09 ·06	-·06 ·06
Pains-taking	-·10 ·06	·02 ·06	·05 ·06	-·01 ·06
Social Status	·05 ·06	-·13 ·06	·12 ·06	·01 ·06

Av. = -·013 ± ·019

It has been found[1] that there is a positive correlation between scholastic intelligence and social status. If imagery is detrimental to the intellectual processes, a negative correlation might be expected between imagery and social status. The average of the correlations in Table III is about 0. No very definite conclusion can be drawn from such a result, as probably the differences in social status in elementary schools are insufficient to bring out any definite relations. It may be, however, as in other cases, that there are none.

7. *Imagery and general ability tests.* At the end of Table IX are added the correlations of the two general ability tests with visual and auditory imagery. As is to be expected from the results already discussed, these are all low.

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[1] "Experimental Test of General Intelligence," C. Burt, this *Journal*, 1909.

TABLE IX.

	Vis. Im.	Aud. Im.
Scholastic Intelligence	−·05 ·054	−·10 ·053
Practical Intelligence	−·02 ·053	−·12 ·053
Painstaking	−·07 ·053	·00 ·054
Social Status	−·02 ·054	·01 ·054
Verbal Memory	·05 ·053	·01 ·053
Vis. D.	·23 ·053	·16 ·053
Aud. D.	·07 ·053	·00 ·054
Vis. M.	·18 ·054	·11 ·053
Aud. M.	·15 ·055	·16 ·055
School Marks	·01 ·075	−·10 ·075
Vis. Im.		·35 ·047
Aud. Im.	·35 ·047	
Opposites	·19 ·069	·18 ·069
Sentences	·13 ·066	05 ·067
Drawings	·13 ·070	·08 ·072
Holes	·06 ·071	−·25 ·072

Av. = ·050 ± ·011.

In the first column of figures in the table below, are given the correlations of the pooled tests (Table IX) with '*g*' the general factor. In the second column are given the percentages of negative correlations with imagery of the tests in each group from which the pooled tests were obtained, that is, the percentage of negative correlations in Tables IV, V, VI, VII and VIII.

	Correlation with '*g*'	Percentage of negative correlation in group
Practical and scholastic intelligence	·63 (average)	75
School marks	·59	34
Visual and auditory memory	·49 (average)	21
,, ,, ,, discrimination	·44 (average)	18
Verbal memory	·37	16

If the two columns of figures are examined, an interesting relation becomes apparent. It will be seen that the larger the correlation with '*g*,' the greater is the number of negative correlations in the group. This confirms the statement which has now been made by several authors, that there is even a tendency to inverse correlation between some of the higher mental processes and visual and auditory imagery.

VI. SUMMARY.

The correlations between the different types of imagery are comparatively high; higher than those of imagery with any other mental performance. It is therefore doubtful if there are any very definite image types[1].

Imagery is probably helpful to a small degree in the processes of discrimination and memory, and visual imagery is slightly more so than auditory. The low correlation of imagery with these processes is not due to the fact that no consideration of verbal imagery has been taken, because certain of the tests were of such a nature that verbal images could not have occurred.

Imagery may be helpful to a small extent in verbal memory, but there is no tendency for memory of visually presented material to correlate highly with visual imagery, or for memory of auditorily presented material to correlate highly with auditory imagery. Even the tests which were designed to correlate with imagery did so only to a slight degree. This casts a doubt as to the value of any objective method of determining image type.

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[1] Prof. Carveth Read says on the subject of imagery and image types: "Plato and Bacon are plainly visiles; but visualisation of the pictorial sort is, after all, never more than an accompaniment or introduction to the kinaesthesis; and whilst in some men it persists, as a sort of generous excess or extravagance of ideation, in others, for greater concentration, it is omitted, like many other links in the economic abbreviation of the organic processes. The depth of the differences between visile, audile, motile, types is easily exaggerated: motility is fundamental in objective judgment" (*The Metaphysics of Nature*, p. 50).

If the various mental performances are considered with regard to their correlation with '*g*' the general factor, it has been found that there is a tendency to inverse correlation between some of the higher mental processes and imagery. Thus in school studies imagery may even be detrimental.

Although these results cannot prove that mental activity may take place without visual and auditory imagery, yet they do prove that the clearness of such imagery bears no relation to the effectiveness of the mental processes which it accompanies. It might be argued that the usefulness of imagery depends upon its mere presence. This, however, seems improbable. It is not usual to find that imperfect tools serve so useful a purpose as perfect ones.

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PUBLICATIONS RECENTLY RECEIVED.

Psychopathology of Every-Day Life. By Professor SIGMUND FREUD, with an Introduction by A. A. BRILL. London: T. Fisher Unwin. 1914. Pp. vi + 338. 12*s.* 6*d.* net.

This book is an authorised translation of the fourth German edition of Professor Freud's popular and well-known work. The topics dealt with in its twelve chapters are the forgetting of proper names, of foreign words, of names and order of words, and of impressions and resolutions; mistakes in speech, in reading and in writing; errors, erroneously carried-out actions, symptomatic and chance actions, and combined faulty acts; childhood and concealing memories; and, finally, determinism, chance, and superstitious beliefs. Owing to difficulties of language, some of the original cases have been modified or replaced by examples more comprehensible in English. The translation itself is of the same degree of merit as the previous translations of Professor Freud's works.

The Circulation and Sleep. By JOHN F. SHEPARD. New York: The Macmillan Company. 1914. Pp. 83. $2.50 net.

The investigations described in this volume were carried out chiefly upon two trephined subjects. The volume of the brain and the periphery, the breathing, the blood pressure, the heart rate and time of transmission of the pulse wave, the jugular pulse and the size and form of the pulse were all studied in combination. An atlas, separately bound, and containing eighty-three plates, accompanies the volume; and gives selections from the numerous records taken. The author finds, with sleep, a sustained and marked increase in volume of the brain, together with a fall of general arterial pressure; with awakening, he finds the reverse. He concludes that there is an active vaso-motor control of the brain vessels in man; and that the brain vessels relax on going to sleep and constrict on awakening. He consequently rejects the anaemia theory; and suggests that sleep may be due to a condition of inhibition produced by sensations of 'fatigue' or 'rest.'

An Outline of Psychobiology. By KNIGHT DUNLAP. Baltimore: Johns Hopkins Press. 1914. Pp. 121. $1.25.

This outline is designed for students who approach psychology with little or no biological training; and is intended to give them the elementary morphological and physiological knowledge indispensable to a proper study of the mind. It is illustrated with seventy-seven figures from various sources; and references for further reading are appended to each chapter. Of nine chapters five are neurological. Two introductory chapters are given upon the cell and the adult tissues of the human body. A most commendable feature is the addition of chapters upon muscle and gland.

An Introduction to Experimental Psychology in relation to Education. By C. W. VALENTINE. London: W. B. Clive, University Tutorial Press. 1914. Pp. vii + 194. 2*s.* 6*d.*

The lack of a small, simple, and trustworthy textbook of experimental psychology, suitable for Training College students, has long been felt. Mr Valentine's book is intended to supply this want. The first part describes some twenty-seven experiments, all of which can be carried out without special apparatus; the second part discusses the results of such experiments and their application to school children. The selection of topics is admirable; and the style throughout clear and interesting.

The Philosophical Review, Vol. XXIV. No. 1. New York: Longmans, Green and Co. 1915. 3*s.* net.

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PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY.

Nov. 7, 1914.	The Connotation of the term Intelligence, by E. J. G. Bradford.
	The Nature of Specific Mental Factors, by Miss N. Carey.
	Simultaneous and Successive Association, by A. Wohlgemuth.
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